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J. XXVI. Dew

ESSAYS

ON VARIOUS SUBJECTS

CONVICTED WITH

MURDER.

BY WM. F. DEWEES, M. D.

PHILADELPHIA: J. B. LIPPINCOTT & CO. 1852.

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J. B. LIPPINCOTT & CO. 1852.

1852.





# ESSAYS

ON VARIOUS SUBJECTS

CONNECTED WITH

## MIDWIFERY.

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BY WM. P. DEWEES, M. D.

MEM. AMER. PHIL. SOC. MEM. PHILAD. MED. SOC. MEM. ACADEMY OF MED. &c.

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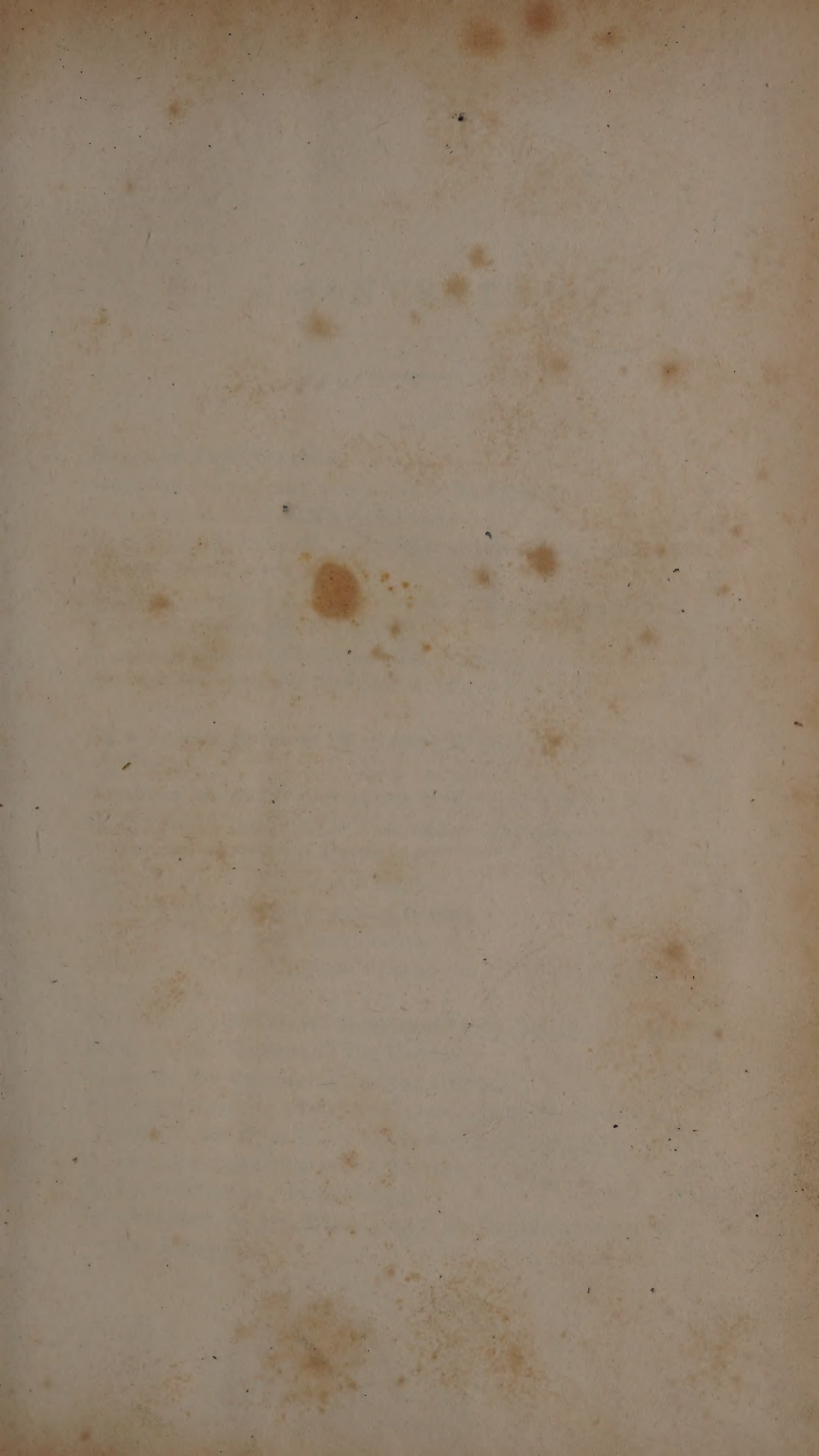
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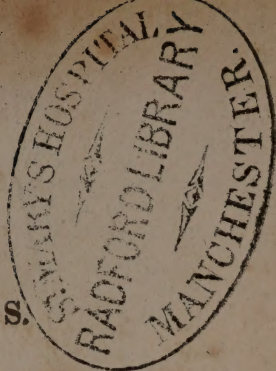
D. CALDWELL,  
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1830. 5. 1.

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## ADVERTISEMENT.

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THE contents of the Volume now offered to the Public were originally published in various periodical works of this city. It was thought they might be acceptable, if they were collected in a body and published under their present form. How far they may deserve patronage, the Public must determine. They are chiefly upon practical subjects, and the observations they contain, are, for the most part, the results of an extensive practice in Midwifery for four and thirty years. The author trusts he does not set an undue value upon his efforts: for no one is more fully aware of the difficulty of writing *usefully* upon practical subjects; nor is any one more sensible of the high responsibility that should attach to effusions declared to be the deductions of a long and extensive experience. If he err, it is the frailty of judgment; for he truly believes that he could not be tempted, wittingly, to mislead.

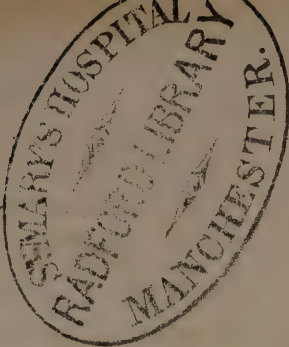
The Essay on Superfœtation occasioned the answers which follow it; which might, at first sight, be considered as imperfect. But this is not so: for the “answers” contain all the arguments or points of controversy that were material to the investigation of the subject: he is persuaded they are fairly stated; but he is not to determine how successfully they are met.

AN ESSAY

ON

## SUPERFŒTATION.

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THE possibility of superfœtation is not a new idea ; the present essay is an attempt to revive it, and to establish its probability, as well by reasoning as by facts. Many cases have occurred since the history of medicine to countenance a belief in it, and it did for a long time prevail ; but, like many other opinions that could not admit of absolute demonstration, it has long since been laid aside, or, in other words, held as a physical impossibility.

It has been urged that superfœtation could not take place, from the indispensable necessity of the male semen passing through the mouth of the uterus to produce conception ; and secondly, that so soon as this event shall have taken place, the os uteri closes, and becomes impervious to the semen ejected in subsequent acts of coition.

If these opinions be founded on facts, the impossibility of superfœtation is established beyond the power of controversy ; but these are the points to be investigated.

Let us therefore inquire into the probability of this theory, and see how well it will accord with facts and reasoning.

Before we proceed, however, any further, let us for a moment consider the anatomy and situation of the unimpregnated uterus : we shall find it a small flattened body floating as it were in the middle of the pelvis ; composed of muscular fibres, nerves, blood-vessels, lymphatics, &c. divided in general by anatomists into body, fundus, and



neck ; having too small perforations near its fundas, which are the passages to and from the fallopian tubes ; with a cavity capable of admitting a bean if its sides were distracted ; but these sides are, for the most part, if not always, when not mechanically stretched by some power or other, in a state of collapse or contact, or at least as much so, as a pretty thick mucus, with which it is constantly supplied, will admit of ; having a neck pendant in the pelvis, which is pervious and capable of admitting a probe, and this, like the body, is also lined with a thick ropy mucus ; the termination of this neck is the *os tinæ*, which has no fixed place in the pelvis or vagina ; it is sometimes found inclining to the right, at others to the left ; now looking upwards and anteriorly, presently dipping downwards and posteriorly ; but most frequently it is found (especially in women who have borne children) lying or resting on the internal face of the perineum ; possessing no power that we are acquainted with, to fix its situation at any time, consequently is subject to all the changes of place that the pressure of the abdominal muscles may give it, when exerted in making of water and going to stool ; to all those that may arise from the weight of the intestines and abdominal viscera ; from the full or empty bladder ; from the distended or flaccid rectum, &c.

After having thus far considered the uterus, let us next attend to what must be effected by the male, that impregnation may take place agreeably to the theory just mentioned. It is, that the male organs of generation must have sufficient vigour to push a thick tenacious fluid through the narrow aperture of the neck of the uterus, and make a lodgment of it within its cavity : can it for an instant be supposed they possess this power ? We think they do not for the following reasons :

1st. Because they have not, in our opinion, sufficient strength for this purpose, if it even be admitted that the extremity of the male urethra and the *os tinæ* of the female were in a state of perfect apposition.

2dly. Because they seldom or never are in a state of apposition, owing to the contingencies just mentioned on the

part of the female ; and also on some depending on the male ; the penis being either so long as to reach beyond the mouth of the uterus, or the urethra so imperfect in its continuance along the penis, as not to reach beyond the labia. We have abundant examples of the former among blacks, and of the latter, *Morgagni*\* gives us some memorable instances.

3dly. Because the male organs do not possess sufficient power, when exerted even to their greatest degree, and when free from all restraint, to effect this purpose ; but their power is constantly diminished, by the vagina for the most part surrounding and embracing the penis pretty firmly throughout its whole length, and by the end of the penis coming in contact with some of the soft parts within the pelvis ; † consequently, the impetus the semen derives from the parts destined to push it forward, must be very much abated ; and its projectile force is not only thus nearly destroyed, but its direction is so altered, that it cannot effect a lodgment within the uterus.

4thly. Because the tenacity of the male semen is such, as renders its passage, through the small aperture in the neck of the womb impossible, even by a power or force much superior to that which we may rationally suppose to reside in the male organs of generation.

5thly. Because the small aperture through which the semen must pass, is constantly lined, or rather filled, with a thick tenacious fluid, which alone would seem to offer an insuperable barrier to its progress, where the penis and os tincæ in the most favourable state of contact.‡

\* Morgagni de causis et sedibus morborum. Epis. xlv. Art. 8, &c.

† This especially happens in women who have had a number of children ; for in them the uterus becomes habitually lower after each succeeding labour, so that their uteri lie, for the most part, just within the os externum : besides, many women are subject to a prolapsus of the womb so that this viscus occupies completely the vagina—yet impregnation takes place with them as readily, as with those who are not subject to this accident.

‡ Besides the reasons just mentioned, we may urge cases, where it was physically impossible for the semen to procure admission into the uterus, through its mouth, by the force exerted on it by the projecting organs. In one instance with which I am well acquainted, the opening of the urethra is not at the extremity of

Some, however, have been determined to overcome every difficulty that may be urged against this direct conveyance of the semen, by supposing the uterus possessed a power of admitting the penis by the opening of its mouth.

The admission, however, of this glaring stretch of probability, will not answer their purpose, unless they also shew us a power whereby the direction of the neck of the uterus may be constantly regulated; sometimes to advance it or make it recede; to elevate or depress it, as circumstances may require: for we have already said they were seldom or never in a state of apposition.

Besides, many cases of impregnation have taken place, where the penis never entered the vagina: a few of which we will relate.

Mauriceaux\* mentions a case of a woman who conceived and was delivered of a child, although her hymen was not broken by coition.

Ruysch† relates a remarkable case of a woman being in labour whose hymen was entire, and against which the child's head pressed and prevented its delivery. He cautiously made an incision through it, and then perceived another thick membrane, this he also divided and the woman was delivered.

Hildanus‡ mentions a case somewhat similar to the two just quoted. He says a young woman in Paris was married, but could not admit the embraces of her husband; in consequence of which he sued for a divorce; but the woman suspecting herself pregnant, was examined by several eminent surgeons, who found the entrance of the vagina shut by a strong, thick, callous membrane, in which were

the penis, but under the glans and on one side of the frænum.—In another with which I am equally well acquainted, the impetus the semen receives, however powerful it may be, is effectually destroyed before it escapes from the canal, by a stricture in the urethra; a considerable time is therefore employed before the semen is discharged, and this is at last only effected guttatim. In both the above cases the wives of these gentlemen bear children: nor is there the least room to suspect their fidelity. In these instances, how was the semen made to pass through the neck of the uterus?

\* Observat. 489.

† Tom. I. Observat. 22.

‡ Centuria III. Observat. ix.



several small openings sufficient to allow of the discharge of menstrual blood. The membrane was divided, and by proper means kept open: the husband was satisfied, and in six months the woman was safely delivered of a full grown child.

Harvey\* says, he “knew a woman, who had all the interior part of the neck of her womb excoriated and torne, by a difficult and painful delivery: so that her time of lying-in being over, though she proved with child again afterward, yet not only the sides of the orifice of the neck of the womb near the nymphæ did close together, but all the whole cavity thereof, even to the inner orifice of the matrix, whereby there was no entrance even for a small probe, nor yet any egress to her usual fluxes. Hereupon the time of her delivery being arrived, the poor soul was lamentably tortured, and laying aside all expectation of being delivered, she resigned up her keys to her husband, and setting her affairs in order, she took leave of her friends. When, behold, beyond expectation, by the strong contest of a lusty child, the whole tract was forced open, and she was miraculously delivered,” &c.

We shall now add another remarkable fact from the same author.†

“The queen,” says he, “had an exceeding white mare, excellently shaped, presented unto her: whose genital parts (lest by going to horse she might endanger the beauty of her proportions, and become unfit for use) were, as the custom is, locked up with iron rings. Notwithstanding which, this mare (by what accident I cannot tell, nor could the groomes inform me) was made big with foale; and at last, when they feared no such matter, she foaled by night, and the foale was found alive next morning by the mare’s side.”

We might easily multiply instances of the like kind, but these we trust will be sufficient to prove that conception has taken place where the hymen was entire, and consequently, where the penis did not enter the vagina to eject semen into the uterus, to form of itself a fœtus, according

\* Harvey Evercit. lxxiii. page 492.

† Harvey. loc. cit.

to the opinion of one set of theorists ; to mix with the female semen as taught by a second ; to moisten the womb, and by its aura impregnate the ovum, agreeably to a third ; nor to travel through the fallopian tubes to the ovaria in conformity with a fourth.

Besides, Harvey and De Graaf dissected animals at almost every period after coition, for the express purpose of discovering the semen, but were never able to detect the smallest vestige of it in the uterus in any one instance.

We are however well aware that Ruysch has asserted, in the most unequivocal manner, that he found the semen in its gross white state in one of the fallopian tubes of a woman, who died very soon after, or during the act of coition.—But we conceive that this able anatomist must have been deceived as to the nature of the substance he found in the tube, and that it was not really semen : our reasons for thinking so are, first, that the semen, after it has escaped from the penis, very quickly loses its albuminous appearance, and becomes as thin and as transparent as water. Secondly, if it be even admitted the semen has effected a lodgment within the uterus, what power exists there to transport it in its original form to the fallopian tubes ? We know of no such power.

It may however be urged, that the fallopian tubes have the power of absorbing, and by this means would be able to take up the semen; and consequently, it might be found in them.

But several important objections may be made to this opinion. First, how will the openings or mouths, if you please to call them so, of the tubes come in contact with the semen, or, in other words, how will the semen get to them, since it must occupy the lower part of the uterus, and consequently be at least an inch from them ? Secondly, the structure of the tube is such, as forbids us to suppose absorption to be a part of their use. Thirdly, it would be assigning two offices to them, diametrically opposite to each other ; first, to absorb and convey the semen to the ovaria ; then to seize the impregnated ovum or ova and

carry it or them to the uterus. Need we say this is absurd? We have no analogy in the human body that we are acquainted with to support it.

We are therefore inclined to think, nay, we are certain, that Ruysch was mistaken: some alteration in the natural secretion of the parts was mistaken for semen; this was nowise difficult for him to do, as he had a particular theory to support—and more especially, as this supposed discovery made so much for it. It is not merely speculative, when we say that some change in the natural secretion of the parts may have been mistaken for semen; for we have the testimony of Morgagni on our side. He tells us he has seen similar appearances in several instances in virgins and others, who had been subject during their lives to leucorrhœa. Ruysch's subject, though not a virgin, may have yet been troubled with this complaint.\*

After having thus, we believe, rendered it more than probable that the semen never passes into the uterus, and in doing this, removed the objections founded on the contrary belief, to the possibility of superfætation; let us proceed and see how we can support the idea of its taking place, when absorption from the vagina is admitted as the means by which the male semen is applied to the ovaria.

This absorption may be effected in one of two ways; first, either by the common absorbents of the vagina taking up the semen and going the route of circulation; or, secondly, by a particular set of vessels which we shall call seminal absorbents, and which have a direct communication with the ovaria. With respect to the first, it cannot well happen in this way, as every thing taken up by the absorbents, is subjected to their alterative influence, and in this case the peculiarity of semen would be immediately lost by the exertion of this power; nor is this difficulty obviated, by supposing this change necessary to the end, since it has been proved by Spallangani that the absolute

\* Morgagni indeed expressly tells us, when speaking of the natural secretion of the fallopian tubes, that it has been mistaken by some for the semen virile.

See *Morgagni Epist.* xxvi. *Art.* 18.

presence of the semen in its unaltered form is essential to impregnation. Therefore, we are inclined to believe it to be in the latter way ; as it would seem to agree better with the general simplicity of nature. No one to be sure has ever demonstrated these vessels (or as far as we know intimated a belief of them :) but this does not do away their existence, or invalidate our opinion of them.\* No one has yet ever shewn the lymphatics of the brain ; yet it is admitted on all sides they exist ; no one has ever traced them on the amnion ; yet there is every reason for supposing them plentifully spread upon it ; no one has ever followed them into the substance of bones, yet we have abundant proof of their constituting a part of them.

We shall therefore, notwithstanding we cannot demonstrate them, take it for granted they exist. We suppose them situated just within the vagina ; some may be even external to it, and just within the labia ; most probably they are in some instances pretty abundant here, as we see conception taking place when the semen could only have been applied to these parts. After the semen has been thrown from the penis into the vagina, it is confined there a longer or shorter time by means of the rugæ ; these rugæ answer a double purpose, first, they serve to retain the semen that it may liquefy and more easily spread over the surface of the vagina ; and, secondly, by their means a much larger surface is offered to be absorbed from. It is more than probable that these are the real uses of the rugæ.† They may perhaps contribute in a degree to increase venereal pleasure, but this is certainly not their only use as some have imagined ; for the doe, according to Harvey, has them in abundance ; and he affirms, she always takes the male with reluctance and seeming pain. Moreover, we see immodest women enjoying the venereal congress, when their vaginæ, from their long con-

\* The existence of these vessels are now rendered almost certain, as Dr. Gärtner of Copenhagen has discovered a duct leading from the ovary to the vagina.

*Edin. Med. and Surgical Journal.*

† Speculations on Impregnation.



tinuance of their debilitating habits, have the rugæ destroyed.

It may be asked, if there be this particular set of vessels within the vagina for the express purpose of taking up the semen, why do they not also absorb the matter of gonorrhœa or lues, and this produce the destruction of the ovaria by conveying it to them? To this we might answer, that, such may be their economy or dispositions, that they are only roused to absorption by their own particular stimulus, namely, the male semen.

This arrangement is not unique; we have many instances of this kind in the animal system; thus, light admitted to the tongue produces no sensation; yet let fall upon the eye, powerfully affects it; the vibration of a musical chord, or the tones of a flute, induce no change on the eye; but the ear is instantly influenced by them. But perhaps a more striking and just example may be taken from the economy of the lacteals of the intestines; they refuse admission to the excrementitious parts of our food, or in other words, are only excited to action by their own proper stimulus, namely, the chyle. It may perhaps be objected here, that various other substances are taken up by them besides chyle, such as the colouring matter of madder, mercury, &c. But we must recollect, that mercury never has been detected in the circulatory system; and Dr. Physick's experiments\* go very far to prove it never is taken up. As to some other substances, we grant they may be, but must believe, that they either are not in sufficient quantity or quality to make the chyle lose its peculiar stimulus. Nay, perhaps the arteries and veins may be justly considered in point; as we think it more than probable, that no other fluid than blood would influence them to carry on the circulation.† And we have arrived almost to a certainty, that no fluid, save the male semen, will in-

\* In a paper read before the Academy of Medicine.

† This has been lately abundantly proved by Dr. Blundell in his experiments on transfusion—indeed, he found that not only blood was required for this purpose, but that it must be the blood of the same species of animal.

fluence the ovaria so as to produce the phenomenon of a conception. It is true, there are instances upon record, of hair and teeth being found in the ovaria of virgins, which might seem to contradict this belief; Dr. Baillie\* and others furnish us with examples of this kind; but in these cases we agree with the Doctor that they are not the produce of conception; since, agreeably to Ruysch,† they have also been found in a man's stomach; if they are thus accidentally produced, they may with as little surprise be formed in the ovaria as elsewhere: we therefore cannot admit them as exceptions to this last position.

Since then we know, that certain parts of the body obey only certain or specific stimuli, why may there not be a set of vessels that are obedient only to the stimulus of the male semen? For our own part we see no difficulty in admitting the idea.

Is not this opinion strengthened, by observing some women who have been barren with their first husbands, prolific with their second, and vice versa? The semen, in these unsuccessful instances, wanted that sufficient or peculiar energy to call the seminal absorbents into action.

Besides, the very sudden effect which is sometimes produced by the male semen upon the female constitution, such as violent sickness, retchings, vomitings, nervous affections, as they are termed, &c. will scarcely admit of explanation, on the supposition that it must go the tedious route of circulation before it arrives at the ovaria to produce its effects. And it will perhaps be difficult to conceive then, how it can be successfully applied to the ovum or ova, as it must still be contained in blood-vessels, whose sides are impervious in the living animal; whereas, the seminal absorbents most probably terminate on the ova, and thus, as soon as fit, will be subjected to the influence of the male semen whenever absorbed.

However, be this as it may, the male semen seems absolutely necessary to the production of the animal, and is

\* Morbid Anatomy, page 265.

† Ruysch, Tom. II. Aversar. Anatom. Decad. tert.

in some way effectually applied to the ovum or ova, and thus produces the phenomena of impregnation.

Should there be but an ovum fit for the male influence, we shall have but one fœtus, if two, we shall have twins, and so on. But for the most part there is only one ; nature kindly providing against the neglect that must necessarily arise from several being produced at a birth.

It would appear in general also, that a regular period elapses between the perfecting of each ovum ; and hence we see women bearing children at stated intervals : for instance, every thirteen or eighteen months ; every two, three, four, five, six, or seven years. Two, three, or four, ova may chance to ripen (if we may so term it) at the same time ; or in other words, may be in a condition to receive successfully the male influence ; then we shall have, as we observed before, a corresponding number of children.

This law of perfecting the ova, however, is not immutable ; there may sometimes happen a considerable variation in the term, but when in a condition, may receive the stimulus of the male semen, and this may happen during the residence of a fœtus in utero ; hence superfœtation. But the time which elapses, for the most part is pretty uniform ; and it would appear necessary also, that the first ovum or ova should be displaced before others can be perfected. This is a wise regulation of nature ; otherwise, women who have lived long single, or been a long time deprived of commerce with man, would be subject to serious inconvenience ; they would be liable to a litter of children. This rule obtains in other animals besides man.\*

Let us suppose now, a fœtus to be occupying the uterus ; the woman to have a subsequent connection with her husband ; the semen to be absorbed and to meet with another ovum capable of being influenced by it ; what will be the consequence ? The ovum will be impregnated, and the ordinary changes will take place in the ovarium ; the ovum will escape into the fallopian tube, and through it pass to the uterus ; here it will meet with a feeble resistance from

\* See Harvey, Spallanzani, &c.



the membranes which already line the uterus, and consequently cover the openings of the tube; this resistance will however be soon overcome; either by the ordinary efforts of the tube, or by the ovum resting unusually long, and beginning to develop, obliging the mouth of the tube to open, while it contracts with unusual violence behind, from the stimulus of distention, and thus forces it forward and displaces the slightly adhering membranes, and by this means will effect a lodgment in the uterus by the side of the other, where it will be as completely developed for the period of its stay, as though it had been placed there at the same instant with the other. It will have its own membranes, water, and placenta; having nothing in common with the other but its nidus.

In confirmation of the above doctrine, we shall beg leave to relate a couple of cases, complete we conceive, in all their parts, to force the belief of the possibility of superfætation; or, in other words, that the cases we shall detail, are really and bona fide cases of superfætation.

## CASE I.

On the 10th October, 1799, at 5 o'clock P. M. I delivered a lady of a fine healthy boy, after a labour of some hours. After a careful delivery of the placenta, I examined my patient by the vagina, and also by a hand upon the abdomen, to discover if there was another child, (for it was supposed by the lady's friends she was pregnant with twins) but could discover nothing like one. She was therefore put to bed, and enjoyed a sleep of several hours: she was roused from this, at length, by severe and regular pains; after they had continued some time, she felt something protruding from the vagina: this gave great alarm to her nurse and friends, and I was immediately sent for. When I arrived I found them in the greatest alarm; they supposing it was the uterus which had passed out. I immediately examined my patient, and found, instead of the

uterus, an ovum complete. I extracted it carefully and entire. Upon opening the membranes, an embryo of between three and four months presented itself, looking fresh and almost transparent; the funis large, white, and shining; the placenta healthy and entire; the blood on its maternal surface rather florid, a proof it had not long been detached from the uterus; the waters clear, abundant and gelatinous; in a word, every thing looked as though the child had just parted with life. Those who are in the habit of seeing abortions, very readily distinguish between those which have just been deprived of life, and those which have parted with it a long time; this bore every mark of freshness. I was therefore much struck with its singularity.

The following considerations will, I think, establish beyond doubt that it was a case of superfœtation:

First, the absence of hæmorrhagy during the whole period of gestation; which would not have been the case, had the placenta been any time detached before the period of labour.

Secondly, the ovum having nothing in common with the full-grown fœtus; on the contrary, it had its own membranes, water, placenta, &c.

Thirdly, the fresh and sound appearance of the ovum.

Fourthly, it having maintained its attachment to the uterus, after the birth of the other child; or at least it did not descend, so as to be discoverable by a careful examination by the vagina and otherwise, which renders its attachment more than probable, since this must and would have happened by the common tonic contraction of the uterus\* after the birth of the other child and placenta; and that the uterus did contract is certain, as no hæmorrhage followed the extraction of the placenta.

\* By conic contraction, we mean that regular and constant contraction whereby the uterus is reduced to its original size, after the distending causes are removed.

## CASE II.

A white woman, servant to Mr. H. of Abington township, Montgomery county, was delivered about five and twenty years since of twins ; one of which was perfectly white ; the other perfectly black. When I resided in that neighbourhood, I was in the habit of seeing them almost daily, and also had frequent conversations with Mrs. H. respecting them. She was present at their birth, so that no possible deception could have been practised respecting them. The white girl is delicate, fair skinned, light haired, and blue eyed, and is said to very much resemble the mother. The other has all the characterising marks of the African ; short of stature, flat, broad nosed, thick lipped, woolly headed, flat footed, and projecting heels : she is said to resemble a negro they had on the farm, but with whom the mother never would acknowledge an intimacy ; but of this there was no doubt, as both he and the white man with whom her connexion was detected, ran from the neighbourhood so soon as it was known the girl was with child.

We might produce other instances of superfætation from the most respectable authorities, such as Aristotle, Harvey, &c. but suppose the above two sufficient, as it ought perhaps to be more a matter of surprise, why it does not more frequently take place, than that it should occasionally happen ; as its occurrence or non-occurrence, entirely depends on the contingency of the sooner or later arriving at maturity, of the ova, and the absorption of semen.



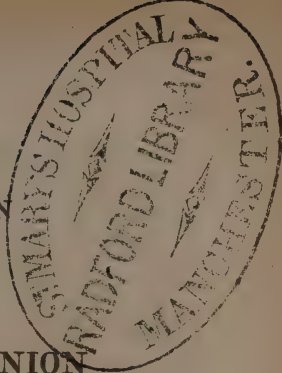
AN EXAMINATION  
OF  
DR. OSBURN'S OPINION

OF THE PHYSICAL NECESSITY OF PAIN AND DIFFICULTY IN  
HUMAN PARTURITION.

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DR. OSBURN, in the introduction to his essay on laborious parturition, has endeavoured to prove, that pain and difficulty are natural to women in parturition. He conceives, "that in sorrow shalt thou bring forth children," was a curse pronounced by God against man, and that it was his intention it should be fulfilled and continued, as long as the world endured. That this curse was felt and perpetuated, by the erect form which he gave man; while the horizontal one of the subordinate quadruped exempted it from these evils; he also supposes, "that the peculiar advantages of positions so different from each other, can no more exist in the same creature, than the strength of the draft-horse and the fleetness of the racer, can be united in the same animal; as these depend on qualities incompatible with each other, and which cannot exist together in the same subject, so those depend on circumstances of structure, or physical laws equally incompatible and inconsistent."

From this it would appear, first, that God intended that woman should bring forth with pain and difficulty; and secondly, that this intention was answered by a physical peculiarity, *that is*, an erect form. From these positions



I must differ. GOD in giving the erect form to man, could not mean it should serve as a balance to the advantages resulting from it to the female ; he intended man for the most perfect, and the most powerful animal ; he gave him faculties, capacities, and appetites, different from all others ; he gave him the erect form as the most dignified, and best calculated to display and improve those transcendent advantages. It would indeed be limiting the power of the DEITY, to suppose, that a mechanism so elaborate, and so perfect as that of man, was necessary to effect a curse (as Dr. O. believes it) so limited. Had this been the intention of GOD, why should not the male participate in its disadvantages ? Or, in other words, why should the female alone incur the penalty ? Since the Doctor himself admits, that, except for this, the erect form is a mark of pre-eminence, and a blessing.

Besides, the physical necessity of pain and difficulty is by no means proved, by the text he has brought forward to demonstrate it. For, "in sorrow shalt thou bring forth children," does not necessarily imply, they shall be brought forth with pain and difficulty ; for sorrow is, in no one instance in the holy writings, made synonymous with pain or difficulty ; in no one instance is it made to signify corporeal sensation :—on the contrary, it is invariably used to express a certain painful state of mind. I therefore believe, it was only intended to express that anxiety which every woman feels for her own safety, and for that of her infant, at the interesting moment of becoming a mother. This state of mind is inseparable from the pregnant woman ; the joyless savage on the banks of the Oroonoko, is not more exempt from it, than the enervated female of civilised society ; yet the former suffers in mind and brings forth "in sorrow," though exempted in a great measure from corporeal suffering. When a woman reflects on what uncertain tenure life is held ; that one-half or more of the human race is doomed to certain death before they arrive at maturity ; the variety of accidents, as well physical as

moral the heir of man is exposed to, she sorrows,—and “in sorrow” she brings forth.

This, I conceive, to be the true meaning of the text quoted by Doctor O.—for were it otherwise, and made to signify pain and difficulty, it would necessarily imply punishment; this punishment ought universally to obtain, agreeably to the intention with which it is said to have been inflicted: but this is not the case; we therefore cannot suppose it was intended as a punishment. On the contrary, it is more than probable that pain and difficulty are artificial, and are the consequences merely of civilisation and refinement. For the human constitution, when not under the influence of these causes, will, *cæteris paribus*, be found capable of meeting and overcoming without any difficulty, the ordinary changes produced by gestation and delivery. Of this, abundant proof might be given; for the female savage, wherever found, whether under the scorching heat of an African sun, or beneath the rigorous sky of the unfriendly Labrador, brings forth her young, without the assistance of an accoucheur or midwife; but the reverse of this almost universally obtains among the females of the civilised world: these differences are most probably occasioned, by the changes produced on the human constitution, by civilisation and refinement.

The mischiefs derived from the sources just mentioned, are found to consist in the disposition to, or existence of diseases, either general or local, or both; in those which may affect the system in general, or those which may be confined to the uterus or pelvis in particular; in the introduction and continuance of certain pernicious customs, habits, or modes of life, thereby inducing a preternatural degree of irritability, sensibility, laxity or rigidity,—and hence the physical necessity of pain and difficulty in parturition, among the greater part of women in a state of civilisation and refinement. The difference, then, in the opinions of Dr. O. and myself, consists in what he supposes natural and unavoidable, I believe artificial, and in part remediable.



I will now examine the Doctor's arguments in favour of this natural physical necessity ; and if their futility or inconclusiveness can clearly be shewn, I trust my point will be established, without the necessity of much positive reasoning.

Dr. O. thinks it as incompatible to unite the advantages of positions, so different as those of man and the quadruped, as it would be to unite the strength of the draft-horse, with the fleetness of the racer ;—yet it is well known that many women bring forth children without pain, consequently the horizontal position of the brute is not exclusively the only one, in which a fœtus may be born without it.

Dr. O. having laid down his favourite positions, namely, that pain and difficulty were intended by the Deity as concomitants on human parturition ; and secondly, that these were effected by the erect form of man ; goes on to consider how this is brought about ; “ to understand,” says he, “ how the erect position of body necessarily operates, in making natural labour in woman more painful, tedious, and difficult, than in the quadruped, it is sufficient to observe, that in such a situation there is the general and powerful influence of gravitation to counteract ; in a certain degree, during the whole period, but in a much greater degree, towards the conclusion of uterogestation ; for as gestation advances, the ability in the soft parts to resist the influence of gravity, regularly decreases : and thus if not prevented, premature labour would be the inevitable consequence.”

“ Completely to guard against this accident, which is of the last importance to the existence of mankind, nature has taken particular pains, and attended to a variety of circumstances in the structure of the bodies, both of mother and child, which, while they effectually answer the purposes intended, unavoidably create those very obstacles which delay and impede delivery. The most natural of these circumstances it may be proper to describe.”

“First, then, that irregular cylindrical cavity in the skeleton, called the pelvis, through which the fœtus in all animals must pass, is so placed in the human body, that its axis is very different from the axis of the trunk, and of course, not perpendicular to the horizon, nor can any thing passing through it, be within the immediate influence of gravity; at the same time, the axis of the pelvis is very remote from, if not directly opposite to, the axis both of the vagina and os externum, through which the fœtus must pass.”

These positions are certainly true; but what do they prove? Certainly not, that pain and difficulty are inevitable to the animal so circumstanced, in bringing forth its young; but merely, that the different axes of the trunk, pelvis and vagina, are not parallel: but this makes nothing for the point, since it by no means follows, that women, to be exempt from pain and difficulty in labour, must have all these axes correspond at one and the same time. This indeed would be a disadvantage agreeably to Dr. O's own confession: for he alleges, that were the influence of gravitation not taken off, premature labour would be the consequence; but this end is effectually answered, by giving this variety to the axis of the parts concerned in parturition; and this, without necessarily being the cause of pain and difficulty, since we see women bring children without them; for the most perfect correspondence takes place successively between the different axes as the labour advances.

“Secondly, upon the same principle,” says Dr. O. “and with the same view, nature has been obliged to vary, nicely and minutely, both the form and the capacity of the pelvis, making it wide in one part, narrow in another, concave and deep behind, straight and shallow before, and with sides that converge to a considerable degree.”

The arrangement here spoken of, though correct, I do not by any means conceive was intended for the purposes Dr. O. supposes, (namely, pain and difficulty,) any more than the one just noticed, since the same argument must

necessarily hold good in both cases, viz. women are delivered without pain, who possess all this variety in their pelves.

“ Thirdly, the upper and lower aperture of the pelvis do not at all correspond in their shape, and have directly opposite diameters. The superior aperture or brim of the pelvis, where the child enters, is oviform, with the long diameter extending from side to side. The inferior aperture, through which the child is to pass out, is so irregular as not to admit of a comparison, or illustration, from any known form, but is certainly shorter from one side to the other, than from the fore to the hind part; and that, in nearly the same proportion as it was longer above: thus the two apertures have directly opposite diameters.”

This construction of pelvis does not, necessarily, produce pain and difficulty in parturition, unless from a wrong position of the fœtus, since the diameters of the head, are made to correspond with those of the pelvis, and this arrangement is essential, for the reasons just mentioned; since by it, two great objects are accomplished; first, the woman is not subjected to abortion when the uterus is impregnated, nor to its prolapsus, when empty; secondly, a resting-place is furnished to the fœtus by the hard and soft parts of the mother, whereby she is enabled to carry her burden to the most remote period of gestation, without any very great inconvenience to herself, or danger to the child.

“ Fourthly, pursuing the same intention, nature has made the volume of the child's head such, compared with the cavity of the pelvis, that it cannot enter by its own weight, but requires the powerful and repeated contractions of the uterus and abdominal muscles, and, even then, the head must be of a particular form, and in a particular direction; that in the passage, both these necessarily undergo a material change from compression, that the shape of the head may be, all through, adapted to the pelvis; and thus it must come out with an altered form, and in a different direction.”

This argument in favour of the physical necessity of pain, &c. can have but little weight, since it amounts to no more than what we have already granted, and are again willing to concede—that, for the head to pass through the pelvis, it must have its large and small diameters to correspond with those of the pelvis. This most frequently obtains, and with such uniformity, that the head might very frequently be still larger, without augmenting or producing difficulty. We cannot here withhold our admiration at the wonderful resources and simplicity of nature in this construction ; that while it affords a security from certain accidents to the animal to be born, it at the same time gives a freedom from evils to the mother, that would be irremediable ; and all this is effected, unfortunately for the Doctor's argument, without necessarily producing pain and difficulty.

The general position of the child's head in utero, is such, that its longest diameter traverses the pelvis rather diagonally ; by this arrangement, portions of the greatest diameter of the head, are constantly presented to portions of the smallest diameter of the superior strait, which prevents the head from engaging in it by its gravity, and, consequently, exempts the mother from some serious inconveniences by this provision.

In brutes there is no necessity for this arrangement ; as their pelves are nearly, or quite horizontal, the axes of their uteri are not exactly parallel with the axes of the pelvis and vagina ; since, the weight of the contents of their uteri carries them below their plane, and consequently, requires as much assistance from the contractions of their wombs to adapt the different axes, as it does in the human subject : but this contraction or effort in the brute is not attended with pain ; we cannot therefore conclude it necessarily productive of it in women. Besides, nearly similar changes must take place in the birth of the inferior animals, as in the human female ; since it cannot be supposed that a similar conformation was given them, for a different purpose ; for instance, in all the pelves of brutes



we examine, we find an inflexion of the lumbar column ; a difference in the diameters of the first strait ; a corresponding difference in the second ; a greater or lesser excavation of the sacrum, and a looking in of the coccyx. The heads of animals, also, bear a very strong general analogy to the human ; such as a difference in diameters, to answer the different diameters of the pelvis ; sutures and fontanelles, in order that the head may conform to the changes, necessarily imposed on it by the contractions of the uterus in its passage through the pelvis. In them also, as in the human subject, do difficulties occur, from a wrong situation of the head, or from an unfortunate presentation of some other part ; in them also do the same consequences result when this happens,—pain and difficulty.

“To add to the more effectual support of the gravid uterus during gestation, all the soft parts concerned in labour, are of a firm and rigid texture, dilating at all times with considerable difficulty, to such a degree as to permit the passage of the child through them without laceration, or other injury. It is obvious that these circumstances must render the act of childbearing, slow, difficult, and painful.”

These last assertions of Dr. O. cannot be admitted, since every day's experience contradicts them. It is a well known fact to every practitioner, that in very many instances, the mouth of the uterus and other soft parts concerned in parturition, yield as kindly with the first child as they do with subsequent ones ; and that sometimes, there is with the first an unwillingness in these parts to yield, that never manifests itself again : besides, these changes take place sometimes so rapidly, and so silently, that the woman is often surprised alone, by the birth of her child. Pain is certainly not necessary to relaxation, nor does relaxation produce pain. Pain may continue violently and for a long time without producing the smallest relaxation ; indeed, it seems unfavourable to it, since it implies more or less disease, or at least, a greater or lesser departure from the ordinary process of nature. Be-

sides, did we admit, that the rigidity of the soft parts and their unwillingness to yield, must necessarily make "the act of childbearing, slow, difficult, and painful," with women in a certain state of civilisation and refinement, it does not follow as a consequence, that this must universally obtain.

Again, it may safely be asserted, that pain and difficulty in those who may experience them, do not in general depend upon rigidity, which Dr. O. considers as a law of nature, and one that firmly establishes his theory; for we find them frequently taking place after the most perfect relaxation; consequently, they cannot be considered as essential to this effect, since the end is answered; and, moreover, relaxation only takes place in the absence of pain, for pain is produced by contraction.

No, it is to the changes already hinted at, as produced by civilisation and refinement, that we are for the most part to attribute the pain and difficulty of human parturition, and not to the peculiar structure of the body. These evils rarely occur among savages, or among those who have not been injured by disease, or perverted by custom. So little trouble has the squaw of this country in her labour, that it never interrupts any project or enterprise she or her husband may have in view; if taken in labour when marching with him, she retires behind a bush, delivers herself, and quickly again rejoins him.

Mr. Swinburne tells us\* that the women of Calabria bring forth their offspring almost without a groan; and that it has become proverbial, "that a Calabrian maid-servant prefers the labour of child-birth to that of a wash."† M. Brydone‡ also informs us, that among the Sicilian women, labour is attended with so little pain or danger, that they appear perfectly well the day after delivery. Many more instances of the like kind might be mentioned from authors, which seem clearly to prove, that this operation

\* Travels in the two Sicilies, page 287.

† Ibid.

‡ Tour through Sicily and Malta, vol. 2, letter 22.

was intended by the Deity, to be performed with ease and safety.

How different is this from what we observe among women, who have been perverted by civilisation and refinement ! What miseries have not our boasted improvements inflicted ! The curse that has fallen upon us has been from luxury, instead of being the fruit of the disobedience of our first parents.

If it should be asked, why pain occurs for the most part, in labours that are so rapid as to employ but a few minutes, I would answer :

That the uterus possesses two distinct kinds of action ; the one, regular and constant, and always tending to diminish its capacity, when its sides are distended, or when the distending force is withdrawn ; it is capable of occasional and powerful augmentation, and, in a natural and unperverted state, is sufficient to effect the delivery of the child. Of this kind, is that action which reduces the uterus to its original bulk after delivery ; of this kind, is that action which effects delivery among females in a savage state, and among those of Calabria, &c. ; of this kind, is that action of the uteri of brutes which relieves them of their burden ; of this kind, is that effort which expels the child after visible life has ceased in the mother :\* this kind of action or contraction of the uterus, is not necessarily attended with pain. This is called the tonic contraction of the uterus. The other action of the uterus is an alternate one, and is, for the most part, attended with pain. This is a distinct action from the other ; and in this instance dependent on it.

This last kind of contraction of the uterus, namely, the alternate, when attended by pain, I am disposed to consider for the most part, if not altogether, artificial or accidental to women ; my reasons for thinking so are :—

First. No physical or absolute necessity for pain ever has been, or ever can be demonstrated.

\* Harvey, Bandelocque, &c.

Secondly. Women in a state of nature, are, for the most part, exempt from it.

Thirdly. If analogy will be allowed to be called in, I can urge, the exemption of brute animals from it, though possessing very similar conformation of pelvis, &c. to the human.

Fourthly. Many women, among those who for the most part have pain in their labours, are sometimes free from it.

Fifthly. It not being essential to delivery, as children have been born after the death of their mothers, by the tonic contraction of the uterus alone; and many women have pains in various parts of their bodies independent of the uterus; as in the jaws, head, knees, &c.

Pain is in very various proportions among women who are equally well formed; we generally find the women of the country more obnoxious to it than those of cities; and the hard-working or laborious part of those in cities, more afflicted than those who live more luxuriously and indolently. Various reasons might be assigned for this difference; I shall however only observe, that, wherever we find that state of fibre which is termed rigid, we shall there find also, *cæteris paribus*, more pain during labour; with this state of fibre there appears to be connected, (or it may exist in this very state,) a greater disposition in the system in general, or in the uterus in particular, to take on what is termed, inflammatory action;—and hence, the utility of blood-letting, and that sometimes to a great extent, in those labours that are attended with rigid *os uteri*, or unyielding external parts; I have frequently seen this remedy act like a charm; it not only hastens the labour, by diminishing the resistance of the soft parts, but also, by the same means, abate pain, as there is now a lesser obstacle to overcome.

From what has been said, it would appear, that the general effects of society and refinement, have produced certain changes on the human female constitution; and that these changes have produced their consequences; which



consequences have given rise and continuance, to pain and difficulty in human parturition.

I will now attend to Dr. O. while he considers "the peculiarities of the quadruped, and their operation in labour."

"By the horizontal position of the quadruped, the parietes of the abdomen support the gravid uterus during gestation, in whatever situation the animal may be; the parts concerned in labour cannot, therefore, at any time, be exposed to the general influence of gravity; on which account, nature was not required to observe such strict laws, or be attentive to such minute deviations, respecting either the position, or capacity of the pelvis, the volume, or form of the head of the fœtus, the situation or structure of the soft parts. Therefore, the same, or very nearly the same axis is given the trunk, the pelvis, the vagina, and the os externum; nature has likewise made the head proportionably small, compared with the capacity of the pelvis; so that it may readily pass through in any direction; and the soft parts, having nothing to support, are of a loose texture, easily yielding to the pressure of the membranes or fœtus, and of course affording little resistance, and no impediment to delivery."

From the horizontal position of the quadruped pelvis, nearly the same consequences result, as in the human female; for the fœtus in them, neither will, nor can be made to engage in the pelvis, until forced by the contractions of the uterus; this is precisely the case in women: and however widely the axes of the uterus, and that of the superior strait may differ before labour, we find a perfect correspondence immediately after, and this is all that is required.

Dr. O. supposes a great degree of rigidity necessary in the uterus, in order that it may support the fœtus; and that this is one of the causes of difficulty in human parturition; but it can be readily demonstrated it is not necessary, even by his own words.

The head, he says, cannot engage by its gravity, since,

the axes of the uterus and superior strait are not the same ; if this be true, how can the head be exactly over the opening of the pelvis, and if it be not exactly over the opening of the pelvis, it must impinge on some portion of its margin ; and if it does impinge on a portion of the margin of the pelvis, what great weight can the uterus have to support ? Thus then we see, rigidity is not absolutely necessary, consequently, must not be considered as a cause, naturally productive of pain and difficulty.

Dr. O. admits, however, that the axes of the uterus, pelvis, and vagina, are not exactly the same in the quadruped : to what is the power given to make them correspond ? It can only be to the uterus : and has not the human female the same agent ? and does it not perform its duties equally well ?

“ The head of the brute fœtus,” he says, “ is proportionably small, compared with the capacity of the pelvis, so that it may readily pass through in any direction ; ”—but upon a strict examination, it will be found to bear the same relation to the different diameters of the pelvis, as the head of the human fœtus does to its pelvis ; and in the latter, it is a well-known fact to accoucheurs, that, it might in general be larger without producing an increase of difficulty ; but the Doctor certainly labours under an error, when he says, that the head of the quadruped fœtus may easily pass in any direction ; since, it is neither consonant with the structure of the parts, nor the mechanism of labour, as daily experience proves.

“ The soft parts in the brute,” he says, “ are of a loose texture, easily yielding to the first pressure of the membranes or fœtus, and of course affording little resistance and no impediment to delivery.” But the Doctor admits they dilate ; so they do in the human subject, and in a state of nature with as much facility ; and this is all that is necessary.

Dr. O. asserts, that it is from this peculiar structure of the soft parts of brute animals, that a laceration of the perinæum never happens ; and also, that is owing to the

rigid structure of the human subject, that it frequently takes place with them.

These assertions of Dr. O. like some others already noticed, are rather unqualified, since it has happened with the cow and the mare, as I myself have witnessed, either from the extraordinary size of the fœtus, or its bad situation. That it is not an unfrequent accident in human parturition is admitted; but it is the effect, in general, of ignorance or inattention, agreeably to the Doctor's own confession. But if it be admitted that this takes place in human parturition, even under the most cautious management, (for Dr. Denman has acknowledged it having happened under his care) what does it prove? Most assuredly not what Dr. O. wishes; on the contrary, it is another support to the position, that the changes produced on the female constitution by civilisation and refinement, are the causes of pain and difficulty in human parturition; and that this artificial state of the perinæum, whereby it is endangered from the passage of the child, is a corroboration; for who is there to guard the perinæum of the American squaw, or the wandering Arab? Yet we hear not of this accident among them.

After following Dr. O. through his principal arguments, it will be proper to advert for a moment, to his conclusion.

“By this sketch of human and comparative parturition, it is evident, why this operation under the most favourable circumstances, or natural labour, must in women be attended with much more pain, difficulty, and delay, than in any other creature.”

Here we find the Doctor again asserting more than he has proved; for if his words, “under the most favourable circumstances,” mean any thing, they must imply—that human parturition must, under all or any circumstances, be attended with pain, difficulty, and delay; this, I trust, has already been shewn not to be the case.

The Doctor then adds, “it remains now to be explained, why laborious parturition never did, nor can occur in the quadruped.”

"It is well known that the great and genuine cause of difficult and laborious births, is the deformity of the pelvis, or the disproportion of its cavity to the volume of the child's head, and that this deformity is caused by a disease peculiar to the human subject, called in infancy rickets, and in the adult state mollities ossium." Dr. O. might have gone farther and have said, that this disease is not only peculiar to the human subject, but to them only under particular circumstances; he has mistaken a consequence for a cause, and by doing this, he has deserted his original positions; namely, that the erect form of the human female, and her peculiar form of pelvis, (independent of disease and the inflicted curse,) were the physical causes of pain and difficulty in her labours. In attributing pain and difficulty to rickets, he appears to forget there are other causes for them; and from its being a disease peculiar to man, draws a most unwarrantable conclusion, "that laborious parturition never did nor can occur in the quadruped."

Dr. O's argument then stands fairly thus; the cause of pain and difficulty in human parturition, is rickets; rickets is peculiar to the human subject, therefore, no other animal can have a painful or laborious labour:—or thus, no animal that is not subject to rickets can have a laborious or painful labour; no quadruped is subject to the rickets, therefore no quadruped can have a painful or laborious labour.

The absurdity of these premises and conclusions are too glaring to need further refutation. I grant, and I believe it universally obtains, that the quadruped is not subject to the disease just spoken of, but I will by no means agree that this exempts them from painful and laborious births; they are by it only freed from this cause of them, for I have seen, the cow particularly, in extreme agony many hours, from a bad position of the head, notwithstanding D. O's bold assertion, "that it is proportionably small, and may pass in any direction."

From what has been said, I trust, it has been made appear that pain and difficulty are not physically necessary.



OBSERVATIONS

ON

DR. DENMAN'S APHORISMS,

ON THE USE OF THE FORCEPS.

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I AM induced to offer the following observations on some of the Aphorisms of Dr. Denman, on the use and application of the Forceps, from a conviction long felt, of their imperfection, contradiction, and ambiguity. I have not attempted to remedy these defects; but have, I trust, from a careful examination of them, pointed out what appeared faulty, and given satisfactory reasons for my dissent. If they should not appear in the same light to others, who may have perused them, I shall be happy to have my own oversights pointed out, and will thank him who may set me right, where I wish not to be wrong.

Dr. Denman has long, and justly, ranked among the first in this department of medicine. The present attempt is not with a view to diminish that fame, by calling in question, without reserve, his talents or his doctrines; it is designed merely to diminish the influence of precepts too limitedly conceived, and too negligently expressed, in a small work, entitled, "Aphorisms on the application and use of the Forceps," &c. Perfection falls not to the lot of man; and Dr. Denman himself acknowledges, that cases for the use of the Forceps occur but rarely in any one man's practice; he therefore did not contemplate a perfect system, in his directions for their use. However

sensible he may have been of the imperfections of his work, from the considerations just mentioned, the public had a right to look for principles which had been well ascertained; and directions without ambiguity or confusion. Situated as Dr. Denman has been, we cannot suppose him ignorant of the many improvements which have been made in midwifery on the continent of Europe; but we fear, that that prejudice, so injurious to the advancement of science, has either made him overlook, or not sufficiently appreciate, the merits of some of the practitioners abroad; how else shall we account for the entire neglect of the principles and practice of Levret and Baudelocque, in the use of these valuable instruments? Nay, his own countryman Smellie, seems to have experienced the same fate, since he, many years ago, established their use and powers much more certainly, than the Doctor appears to think they possess at this day: be this as it may, I have nothing to do with the cause of failure or imperfection; and am only to judge of the work as it stands; but cannot, however, withhold saying, I think its utility very limited or questionable, and its chance of doing mischief great. Knowledge of every kind must be progressive; but with a view to this, errors must be pointed out, or remedied; otherwise it becomes stationary, or perhaps retrograde. It was with a hope that one of these purposes would be effected, that the present strictures were made; and that, by pointing out error, I should, however remotely or indirectly, diminish some of the difficulties or embarrassments attending the use of these truly useful instruments; and also, lessen some of the prejudices entertained against them.

The indiscriminate use of instruments cannot be too seriously deprecated; and with this view, Dr. Denman has laid it down as a fundamental principle, "that no instruments are to be used in the practice of midwifery; the cases in which they are used, are therefore to be considered merely as exceptions to this rule."\* From this conside-

\* Aph. i. p. 13.

tion alone, we ought to have the most clearly defined cases, where, and when, to employ them, as well as the most precise directions for their application ; neither of which ends are answered by the Aphorisms of Dr. Denman. For it must be again declared, they ill define the cases where they are necessary, and vaguely direct the mode of application. After having said thus much, I will give my reasons for my dissent. With this end in view, I will proceed regularly with the book.

The Doctor begins with an arrangement of labours ; which he classes under four general heads, viz. "I. Natural. II. Difficult. III. Preternatural. IV. Anomalous or Complex."

#### "CLASS I. NATURAL LABOURS."

*Character.* "Every labour in which the process is completed within twenty-four hours : the head of the child presenting, and no adventitious aid being required."

Many objections might be made to this character of Natural Labour, which is taken from time and the presenting part ; but as this is not a material point, and as some objections may perhaps be made to any definition that may be given, I shall pass this without comment : only observing, I cannot help preferring Baudelocque's, which makes all labours natural, that do not require the interference of the Accoucheur.

I cannot, however, so silently pass by his "Varieties" of Natural Labour : he reduces these to four, viz.

- "1. The face inclined towards the sacrum."
- "2. The face inclined towards the ossa pubis."
- "3. The head presenting with one or both arms."
- "4. The face presenting."

In this arrangement, we not only find incorrectness, but ambiguity. Incorrectness—since, with the presentation of the head, that of the face is included ; and also, those cases, in which the vertex presents originally to the pubes or to the sacrum, are omitted. These are not distinctions

without differences ; for I hold them important in a work like this, as will be more fully shewn in the sequel. Ambiguity—since we are at a loss to tell what ought to be meant by the head of the child. It has been usual with most authors when speaking of head presentations, to mean the vertex as the presenting part ;\* and they have, for the most part, carefully distinguished between the head and face cases. This distinction is founded on practical necessity, and it is one that the student or young practitioner ought ever to bear in mind, as their mechanism, and for the most part their treatment, are essentially different. Indeed, a little after, the Doctor appears to concede this opinion, since he makes a face presentation a cause of difficult labour.† It is very possible, it is true, that a labour may, agreeably to the Doctor's definition, be natural when the face presents, but this can only happen from the concurrence of several causes, which, but too frequently are absent : in general they are productive of difficult labour, and not unfrequently are obliged to be made preternatural.

## “CLASS II. DIFFICULT LABOURS.”

“*Character.* Every labour in which the process is prolonged beyond twenty-four hours, the head of the child presenting.

The same objections may be made to this definition as to the former ; we find in it the same want of precision as we noticed in the other.

We might dispute, perhaps with great propriety, the agency of some of the causes he has enumerated of difficult labours ; such as the partial action of the uterus ; the imperfect discharge or dripping of the waters ; the shortness of the funis, &c. but, as these are less important mis-

\* “That part of a child which descends lowest into the pelvis, is to be esteemed the presenting part.” Aph. p. 6.

† P. II. ver. 6.



takes, than those proposed now for consideration, I shall pass them by, and immediately proceed with the observations on the "directions for, and admonitions in, the application and use of the forceps."

"The intention in the use of the forceps is, to preserve the lives both of the mother and child, but the necessity for using them, must be decided by the circumstances of the mother alone."\* I consider this aphorism very deficient, or perhaps more properly speaking, very faulty; it is acknowledged, that "the intention in the use of the forceps is, to preserve the lives both of the mother and child;" but we are immediately after told, "the necessity for using them, must be decided by the circumstances of the mother only:" did we always literally obey this precept, it would be impossible to fulfil the indication for which the forceps were prescribed. It is a glaring and palpable solecism, indeed, we had nearly said Irishism; for it seems to declare, you are to save by the forceps both mother and child; but if there is no "circumstance" on the part of the mother that would require their use; that is, (as I understand it,) if she herself is in no danger, or can eventually, no matter how long, expel the child, you must not use them, however important they may be to the preservation of the child. Need it be said, if this advice be followed, it would prove the destruction of very many lives? for instance, when the head is very far advanced, and the vertex is about to emerge from under the arch of the pubes, but is retained there, by the extraordinary size of the child's head, by its uncommon ossification, or the unusual rigidity of the external parts; ought we run the risk of losing the child by withholding the forceps, because the indication is not taken from the circumstances of the mother? Or, let us suppose the body and shoulders of the child to be delivered, and its head to be retained at the inferior strait; in this situation, it would inevitably perish were it to continue long, nay, but a short time:

\* Sect. i. Aph. iv. p. 13.

ought we to abandon the poor infant to its fate, because its head may have been either positively or relatively too large, to be immediately expelled by the common efforts of the mother? or because, there was no "circumstance" on the part of the mother, that required this immediate interference? The use of the forceps under "circumstances" of this kind, has been considered by Baudelocque, as a valuable improvement in midwifery, and he does not fail giving the very justly merited praise to Smellie for the discovery.

Aph. 5. "It is meant, when the forceps are used, to supply with them the insufficiency, or want of labour pains; but so long as the pains continue, we have reason to hope they will produce their effect, and shall be justified in waiting."

This aphorism, like the one just noticed, would lead the young practitioner into great error; an error, on which the life of both mother and child is staked. By it, we are authorised to wait as long as pains continue, before we use the forceps; than which, nothing can be more dangerous; by doing this, we lose time truly precious to the individuals concerned. How many cases are there, where the forceps might not only be used with safety and advantage, but where they are truly indispensable, yet, where pains continue, nay, even continue with violence, but unavailingly, either from the bad situation of the head; from absolute or relative narrowness of the pelvis; or uncommon rigidity of the soft parts: under "circumstances" like these, we are, through false principles, to subject the woman to all the consequences of the long and violent pressure of the child's head on the soft parts within the pelvis, and thereby hazard inflammation, suppuration or gangrene; while we expose the child to all the evils arising from its head being long and violently compressed, by the reiterated contractions of the uterus. Yet here we are forbidden the aid of the only means, by which the labour can be terminated with safety to mother and child, or at least, we are told it will be justifiable to wait, maugre the

accidents that may ensue. Besides, the Doctor has in one member of this aphorism declared, the "forceps are to supply the insufficiency of pains;" if this mean any thing, it must be understood, that, where pains are unavailing, though they still continue, we are to have recourse to other means, than the common or natural agents of delivery; for I presume, the sufficiency of pains ought to be determined by their effect, rather than by their apparent force. Yet immediately after, we are told "so long as the pains continue, we have reason to hope they will produce their effect, and shall be justified in waiting." The inexperienced practitioner would be in doubt what plan to pursue; the timid one, would be lulled into a security fatal to his charge.

What would be the consequence of this indecisive conduct, in cases of convulsions on the accession of the pains? Death most probably to both mother and child; for we have no hope in many cases, but in the speedy termination of the labour, and the forceps are frequently the only means, by which this end can rationally be obtained. What would be the result, did we exclusively rely on the hope, that the pains would eventually "produce their effect" when the head was badly situated at the inferior strait, and where, of course, the proper relation between the head and pelvis does not obtain? Ought we to abandon the unfortunate mother to the unavailing efforts of nature, and become the idle spectators of her fruitless endeavours, because she is still able to continue them; or ought we promptly to step in to her aid, employ the forceps, rescue the devoted child, and save perhaps a valuable mother? Instances might be multiplied, in which it would be equally improper to trust to "time and patience"\* as remedies for difficult labours; but these for the present will suffice.

Aph. 9. "A rule for the time of applying the forceps has been formed from this circumstance; that, after the

\* Page II. Art. i.

cessation of the pains, the head of the child should have rested six hours in such a situation as to allow the use of the forceps."

To wait six hours in many instances, as proposed by this aphorism, would be waiting a time, at the expiration of which, our efforts would no longer be useful or availing. In convulsions, hemorrhagies, and faintings, the most prompt aid is frequently required; to wait in cases like these six hours, is sometimes to seal the doom of our patient. It may, perhaps, be said, it is to be understood, we are to wait six hours, only when no threatening symptom attends; this perhaps may be the Doctor's meaning, but it must be confessed it is not expressed. But if we even admit this, which, by the bye, I consider entirely gratuitous, we still have to complain of great want of precision in this aphorism; it is regulating the accoucheur's conduct by time instead of circumstances, which, I think, no small fault: besides, "threatening symptoms" may be considered as a very indefinite direction; for what one might consider as such, another would not; the nature of those circumstances ought to have been clearly pointed out, where the Doctor would have us act, and where we might safely, agreeably to his opinion, be idle. But this he has not done, and he leaves our conduct to be regulated by a definite number of hours. It does not appear in any instance with Dr. Denman, either in his introduction to midwifery, or in the work before us, that he considers the long pressure of the child's head on the soft parts of the pelvis, as entering into, or making even a part of, an indication for the use of forceps. Whence this happens, we cannot pretend to determine; but certain it is, many others whose reputation is equally high, and on whose judgment at least an equal value should be set, have viewed this matter in a very different light. It has with some been considered as an indication exclusively sufficient, for having recourse to adventitious aid; fearing, much more, the ill consequences of delay, than any mischief that might ensue, from



what Dr. Denman might think a premature application of the forceps.

It is true, the Doctor endeavours to obviate the objections which he foresaw must be made to this rule, by framing Aphorism 10. "But this, and every other rule, intended to prevent the rash and unnecessary use of the forceps, must be subject to the judgment of the person, who may have the management of any individual case." But to whom is this aphorism directed? Is it not giving a discretionary power to those who are supposed not to have sufficient knowledge to direct it? for this work, agreeably to his own declaration, was designed "for students,"—and they, unfortunately, may be called to cases, before they possess that judgment he inclines to rely upon. They, in vain would seek information from this work; they must turn from it with indecision and embarrassment. In a work so professedly didactic, the plainest and most undeviating rules ought to have been laid down; to rely on the judgment of the practitioner is not teaching, it is supposing the possession of a knowledge this work ought to have been capable of giving; instead of obtaining information, we are supposed already not to stand in need of it; instead of being enlightened we are bewildered; on the one hand, we are cautioned against the use of the forceps, vaguely and injudiciously; on the other, we are warned against the too long delay of them, without being clearly instructed where it may be proper to employ them.\*

Aph. 13. "The lower the head of the child has descended, and the longer the use of the forceps is deferred, the easier will in general their application be, the success of the operation more certain, and the hazard of doing mischief less."

How ill does this aphorism comport with the one just quoted? This rule not only restricts us to very narrow

\* Aph. ii. p. 15. "Care is also to be taken that we do not, through an aversion to the use of instruments, too long delay that assistance we have the power of affording with them."

limits in the use of the forceps, but decides without sufficient caution, that the longer they are deferred the better. One of the greatest improvements that midwifery has received for this last century, has been perhaps the great perfection the forceps has been brought, and the certainty to which we have arrived in their application; together with the knowledge of their utility and safety, in almost any situation of the head within the pelvis. Levret, Smellie, Baudelocque and others, bear testimony in their favour, under circumstances not dreamed of by their predecessors, or thought impossible by some of their contemporaries. Before these instruments underwent the valuable changes, effected by the authors just mentioned, and when the pell-mell mode of application was only pursued; this aphorism, might have been adopted in its fullest latitude, without perhaps subjecting our patients to worse consequences than might have resulted, from the forceps themselves. But when we find them in the state of perfection they now are; and when we reflect on the certainty of the laws by which their use and application are directed, I cannot withhold saying, this aphorism teaches not only a false, but a dangerous doctrine. I do not hesitate to agree with Dr. Denman, "that the longer the use of the forceps is deferred, the easier will in general be their application," but I cannot by any means concede "that the success of the operation" will be "more certain, and the hazard of doing mischief less." In contemplating the success of an operation, we ought certainly to consider, whether it is performed at a time, and in a manner that will the most certainly fulfil the objects for which it was intended. Let us see how far the doctrine inculcated by this aphorism can be justified by this rule. The end proposed by the use of the forceps is, to save both mother and child.\* To fulfil this intention, the aphorism tells us, the longer their use is deferred, the more successful will be their operation, and the danger of doing mischief lessened.

\* Aph. iv. p. 13.

Did we follow this advice, we should not apply the forceps until the head was about to emerge from under the arch of the pubes, in cases where the pains propel it from time to time ; and this confessedly would be delaying their use to the greatest possible extent we can suppose ; but to have deferred them until this time, would be contrary to the Doctor's opinion of their use, since he says, aphorism 17. p. 16, " when the head has emerged under the arch of the ossa pubis, the use of the forceps can very seldom be required ;" here we find the Doctor at variance with himself : but, let us suppose the head has not been made, by the repeated efforts of the uterus, to pass through the inferior strait, but merely to have arrived at it, and is there detained by some cause or other ; we must now delay the use of the forceps, until the woman's strength is exhausted in vain efforts to propel the head ; until inflammation or gangrene has ensued ; or, until the child loses its life ; because we are taught to believe, that " the lower the child has descended, and the longer the use of the forceps is deferred, the easier will in general their application be, the success of the operation more certain, and the hazard of doing mischief less." What danger can ensue under circumstances like these from the use of the forceps, that will not be exceeded by their too long delay ? Besides, women are subject to a variety of diseases during labour, that leave no resource but in the speedy application of the forceps ; such are convulsions, syncope, floodings, strangulated hernia, &c.—in cases of this kind, a moment's delay may be fatal. It perhaps may be urged, that, under circumstances so imperious, we have a choice of remedies, and, that turning would be the preferable practice. This certainly is the case sometimes, and would perhaps be always, could we command the conditions necessary for its success ; but this is impossible, and we are left with no alternative but the forceps. For when the head has escaped from the orifice of the uterus, turning must not be thought of ; and when the waters have been long discharged, it is for the most part impracticable. To obey this aphorism, then, is

to wait until the forceps are for the most part useless ;\* or if we do not wait until they are useless, we employ them with uncertainty and hazard, or in waiting for the head to pass to a certain part of the pelvis, that we may employ them with more certainty and success, we subject both the woman and child to danger by ill-timed delay.

From what has been said, it would appear, that, very often by delay, we should not fulfil the object proposed in the operation ; namely, the safety of both mother and child ; and by it, lose the best possible time for performing it.

It will, perhaps, be said, in a work of this kind, some general rules are all that is to be expected, and that the operator must supply its defects. I did not expect rules that would apply to every possible case, but had a right to look for those that would prevent any great error in their application. In this I have been disappointed, and am not singular, as it has not been an unfrequent thing to hear these aphorisms complained of.

Aph. 14. "The forceps should always be applied over the ears of the child ; it must, therefore, be improper to apply them when we cannot feel an ear."

I cheerfully subscribe to a position entertained in this aphorism ; viz. "the forceps should always be applied over the ears of the child ;" but can by no means consent to the deduction, that "it must therefore, be improper to apply them when we cannot feel an ear."

There are many cases, wherein it would be proper to apply the forceps, where an ear cannot possibly be felt ; for instance, first, when the head has not completely passed the superior strait, and before it has escaped from the orifice of the uterus, and, more especially, when this viscus is contracted firmly round the head ; secondly, when the head is wedged diagonally in the inferior strait, owing either to the absolute or relative want of size in the pelvis ; thirdly, when the head presents originally at the superior

\*.Aph. xvii. p. 16.



strait with the vertex to the pubes, and the anterior fontanelle or bregma to the sacrum ; fourthly, where the anterior fontanelle or bregma, presents originally at the superior strait, towards the pubes, and the vertex towards the sacrum ; fifthly, where either of these presentations have passed the superior strait in the direction just mentioned, and has arrived at the inferior strait ; sixthly, and lastly, where the vertex or forehead are about to emerge from under the arch of the pubes. In all these cases, it is impossible to feel an ear by a common examination, (which is the Doctor's criterion of the manageableness of the case) yet, in either of them, the forceps may be exclusively indicated. What could have led Dr. Denman to the framing of this aphorism is difficult to say ; certain it is, it has neither the advantage of simplicity, nor the truth of experience to recommend it.

Let us suppose a case in which the ear cannot be felt, not because it is out of reach of the finger by a "common examination," but because there is not room for the finger to pass, (this let it be remembered is not a gratuitous case, for Dr. Denman himself admits it\* ; ) what must be done in this instance ? every consideration and circumstance on the part of the mother and child call loudly for immediate assistance ; this can only rationally be given by the forceps ; but they must not be used ; why ? because truly, an ear cannot be felt. Thus, then, by a careless distinction, or an unnecessary and injurious caution, the lives of both

\* "It must, therefore, be improper to attempt to apply them before an ear can be felt, either because the head is too high to allow us to reach that part, or because it is so closely locked in the pelvis, that there is not room to pass the finger for that purpose, between the head of the child and the pelvis." Introduction to Midwifery, p. 134.

It would seem, the Doctor did not consider the difference between the thickness of the finger and the blades of the forceps, or that they could very readily pass when the finger could not ; or else he knew of no other method than the ear, to determine the situation of the head, and would not run the risk of being foiled with them, since he could not determine to what part of the pelvis they were directed. What is to be expected, under circumstances like these ? Either that the woman must be allowed to exhaust herself ; the child perish before she can deliver herself ; or have recourse to the crotchet.

mother and child, are to be put in jeopardy. I deem this aphorism of sufficient importance, farther to observe, that it is not only defective for the reasons already given, but also, because it leads the pupil or young practitioner into a very important error ; it obliges him to determine the situation of the head by the ear, and makes him place his dependence for this knowledge on that part ; a mode of acquiring this very necessary information, that is not only very inconvenient, but is also very limited ; nay, it has been already observed, it is impossible sometimes to obtain an idea of its situation by the ear, so that he is either obliged to abandon the use of the forceps, and trust to the efforts of nature, or have recourse to the crotchet ; or he applies them at the risk of embracing the greatest diameter of the head, or diagonally. In the first instance, all the force he could apply, would not be sufficient to deliver the head ; and in the second, they would either not lock, or else would slip, if made to do so by force : in either way the accoucheur must be foiled, and the woman and child suffer. I shall dismiss this aphorism, with a recommendation to the pupil or young practitioner, to depend upon the fontanelles and sutures, for a knowledge of the situation of the head ; they can always be felt, and, if carefully examined, will never mislead.

Having thus noticed the principal aphorism which relate to the employment of the forceps ; I shall now proceed to those which relate to the mode of application. Before we proceed, however, with this examination, it strikes me as necessary to lay down a few rules, the observance of which, I consider as indispensable in the application of the forceps. By proceeding in this way, I shall be saved needless repetition ; and it will, at the same time, serve to shew, at one view, how very deficient Doctor Denman's rules are for the application of the forceps, and how very inadequate they are to the end proposed.

I shall not enter into a minute description of the forceps as we find them at the present day, as they all present the same general character. They differ more in length than

in form, and may safely be divided into the English short, and the French long, forceps. These differences in length, however, make none that is material in their application, as the same general rules apply with equal propriety to both.

I shall describe the forceps as consisting of two blades with handles ; as having a superior concave, and an inferior convex edge ; of having a lateral or external convex, and a lateral or internal concave surface. The blades are distinguished into right and left hand blades. Having given this outline of the forceps, I will proceed with the rules for their application, &c.

#### RULE I.

The forceps are always to be applied over the ears of the child, but in such a manner, as to embrace the head rather diagonally than perpendicularly ; that is to say, the forceps must traverse the head nearly in the direction of a line, if it were drawn, from the vertex to the chin, and not in the direction of a line drawn from the base of the scull to the summit of the head.

#### RULE II.

They are to be applied in such a manner as, that their superior or concave edge may always come under the arch of the pubes, when the head is about to emerge from the pelvis.

#### RULE III.

Each blade of the forceps must have its definite situation in the pelvis, that rule the second may never be infringed.

Having laid down these simple, but important, rules, I shall examine the Doctor's, and see how far they will accord with them.

"The following rules are given," says the Doctor, "on the presumption that the head of the child presents, with the face inclining or verging towards the hollow of the sacrum, &c.

SECT. II. p. 17.

1. "Carry the fore finger of the right hand to the ear of the child."

This rule may be complied with wherever it may be practicable. I have already remarked how seldom this is the case.

2. "Then take the blade of the forceps to be first introduced, by the handle, in the left hand, and conduct it between the head of the child and the finger already introduced, until the point reaches the ear."

The rules we have just mentioned, are, agreeably to Dr. Denman's avowal, "predicated on the presumption that the head of the child presents with the face inclined or verging towards the hollow of the sacrum." The head, after it has arrived at the inferior strait, may be so situated, as, that the face may have two inclinations towards the sacrum, which are essentially different. First, it may be so situated as that the vertex shall be behind the left foramen ovale, while the face will nearly correspond with the right sacro-iliac symphysis; or, secondly, it may be so placed, as, that the vertex will be behind the right foramen ovale, while the face will be towards the left sacro-iliac symphysis. These situations of the head are by far the most frequent which occur in the human subject. The first step necessary, that the forceps may be applied with facility and success, is, to determine which of the two it may be; after having satisfied ourselves on this, head by a careful examination of the fontanelles and sutures, we



are to take care that rule the third is complied with, that we may fulfil rule the second.

Now let us suppose the head to be situated after the first manner; then let us see what rules will be necessary that the forceps may be properly applied; and then attend to the rules of Dr. Denman, and determine how far they will answer the end proposed.

In the proposed situation of the head, the right ear of the child will be rather to the right of the symphysis pubes, and the left, towards the left sacro-iliac junction, the face consequently will be towards the right sacro-iliac junction. That rule the first may be strictly complied with, the right hand blade of the forceps must be nearly behind the symphysis pubes,\* and the left hand one nearly before the sacrum,† while the handles must be much inclined towards the left thigh of the mother. This inclination of the handles is essential to the success of the operation; (not a word of which is mentioned by Dr. Denman) for if we do not attend to this, we must necessarily seize the head in the direction of its perpendicular diameter, instead of its oblique. If we seize the head after the first manner, we shall be in danger of making the face recede too far from the breast, and thus make the head present its greatest diameter parallel with the smallest diameter of the inferior strait: and in order that we may embrace it after the second, and proper manner, the rule of inclining the handles towards the left thigh of the mother, is indispensable.

Let us now examine what would be the consequence did we follow Dr. Denman's rules, in a case of this kind. First, we should embrace the head in its perpendicular diameter;‡ secondly, we should make the convex edges of the forceps come under the arch of the pubes,§ if it were

\* But rather to the right of it. † But rather to the left portion of it.

‡ For we are directed to carry the first blade until the lock reaches the external parts near the inferior edge of the ossa pubis, while the second is to be carried till the lock reaches the perinæum, or even presses it a little backward. Aph. vi. vii. p. 18.

§ For we are directed to place the left hand blade behind the pubes, and the right hand blade between the child's head and perinæum, consequently in this

possible to make the head emerge from there ; or thirdly, should we not bring the convex edges in this manner, we should be obliged, were we to bring them otherwise, to make the face of the child come uppermost.

The presentation we have chosen to illustrate our point, is by far the most frequent of the two mentioned ; but Dr. Denman's rules can only be useful in the second. Besides, there are other situations of the head, in which these rules would apply with even less propriety. I mean where the ears of the child are turned towards the sides of the pelvis. I have purposely left the examination of two aphorisms, (which, in order, should have come before,) until now, as I can at one and the same time, point out their fallacy, and expose the imperfection of his rules for the application of the forceps.

Dr. Denman, in aphorism xvi. p. 16. says, " The ear of the child that can be felt, will be found towards the ossa pubis, or under one of the rami of the ischia," and in aph. xvii. that " the ears are not turned to the sides of the pelvis, till part of the hind head had emerged under the arch of the ossa pubis, when the use of the forceps can very seldom be required."

I have already adverted to Dr. Denman's omission of several material presentations of the head, and specified two, deemed important ; I shall again insist on this here, and show that both the above aphorisms are far from practical truth. The two instances of presentation I noticed some time since, as not having a place in Dr. Denman's varieties of natural labour, were, first, when the vertex or posterior fontanelle is to the pubes, and the anterior fontanelle or bregma is to the sacrum ; and secondly, where the bregma is to the pubes and the vertex to the sacrum. In these presentations the ears are originally placed toward the sides of the pelvis, and the head advances with them in this way, until it arrives at the inferior strait : consequently, in neither of these cases can the ear be felt

presentation, the concave edges of the instruments will be towards the face of the child. Aph. ii. 7.

“under the rami of one of the ischia,” and are, therefore, turned toward the sides of the pelvis, before a part of the hind head has emerged from under the arch of the pubes. In labours of this kind, where the forceps may be necessary, what information for their use can be derived from the aphorisms?

The other aphorisms belonging to this section are common-place, but proper always to be kept in remembrance; we have, therefore, only noticed those we deemed essential. Those of section III. though subordinate to the second, fourth, and seventh, of section II. are judiciously conceived and well expressed.

ON THE  
EFFICACY OF BLOOD-LETTING

IN RIGIDITY OF THE OS EXTERNUM.

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CASE I.

On the 12th September, 1798, I was requested to visit the wife of Samuel Griffith, in consultation with Dr. Jones. Mrs. G——, I was informed by the Doctor, had been in labour sixteen hours; the waters were evacuated early in her labour; her pains frequent and brisk; but, there was not the least disposition in the soft parts to dilate.\*

I sat down to examine our patient, and found the os externum scarcely large enough to admit the finger, and it was mounted up closely against the symphysis pubes, in consequence of the perineum being very much distended by the head of the child. The os uteri was rigid and but little opened—a kind of bridle or small column of flesh ran from the inferior edge of the os pubis and lost itself in the perineum below; against this the head was firmly

\* This patient, like the one whose case was formerly given,† had suffered a laceration of the perineum to a very great extent; the parts, after a considerable lapse of time, healed up, but so unfortunately, as to almost entirely obliterate the vagina. I was called upon for my advice when in this situation, and found the case a truly distressing one; the passage of the vagina was so contracted as not to exceed in size a common quill; the parts extremely callous; and a constant and profuse discharge of fetid acrid pus, kept the poor woman in a continued state of misery and ill health. My friend Dr. Physick was also consulted; by a persevering use of sponge tents, &c. the parts became sufficiently dilated to admit imperfectly the venereal congress; soon after she became pregnant, and the consequences of this pregnancy furnish the above case.

† See *Essay on the means of lessening pain.*



pressed. The head was situated naturally, and so far advanced, that the vertex was about to emerge from under the arch of the pubes, covered with the uterus; it had been thus fixed for nearly six hours previously to my seeing this patient; and all that had been done, was, the occasional exhibition of tinct. opii with steady pressure against the perineum to prevent the escape of the head through it. In this situation of affairs what was to be done?

My ingenious and much lamented friend Dr. E. Smith of New York, immediately after the receipt of my former case, suggested the trial of an infusion of tobacco in similar cases, to supersede the use of such extensive bleeding as had been employed in it; affirming, its effects were very similar to those produced by copious blood-letting—such as nausea, vomiting, syncope, and consequently relaxation. The idea pleased me, and I was determined to employ it the first opportunity; the case under consideration I believed to be as favourable a one as could occur, and accordingly proposed it to Dr. Jones; he cheerfully acquiesced in its trial; a strong infusion was made of the tobacco, and a quantity of it, with some difficulty, after several ineffectual trials, was thrown up the rectum. It produced great sickness, vomiting, and fainting; but the desired relaxation did not take place; we waited some time longer, but with no better success. In the course of an hour or an hour and a half, the more distressing symptoms produced by the infusion wore off, and resolving to give the remedy every chance in our power, we got our patient, with some difficulty, to consent to another application of it. Its effects were as before—great distress—without the smallest benefit; the soft parts remaining equally rigid, as before its exhibition.

Supposing the bridle just spoken of might have some influence on the development of the external parts, I divided it, but without any evident good resulting from it. I now proposed the remedy that had so completely succeeded in a former case; bleeding nearly to fainting: this was consented to. We had our patient placed on her feet;

taking care to have the perineum well guarded during the operation. Upon taking away about ten ounces of blood, she became very faint;\* she was immediately laid upon her bed; the most complete relaxation had taken place; the forceps were applied, and our patient was delivered in a very few minutes of a fine healthy girl. The mother was put comfortably to bed, and every thing went on in the ordinary way until the sixth day, when she was seized with a violent cholera morbus and convulsions (to which she was subject) and died in twelve hours.

This case, notwithstanding its ultimately unfortunate termination, fully establishes the influence of blood-letting in this very distressing kind of rigidity; and proves it to act differently from tobacco, notwithstanding the latter produces nausea, vomiting, and syncope; and also, that the quantity of blood lost, in some instances, may be very small, to induce the desired relaxation.

We conceive, that no possible blame can attach to the bleeding in this case, as the woman was very brave until the sixth day, when a disease, to which she was subject, supervened, and carried her off.

\* The subject of this case was a delicate woman, and wont to become very faint upon the loss of a little blood. The disposition to syncope has frequently aided me in obstetric cases, where this state was an object in practice; it is well to inquire if this peculiarity exists, as, taking advantage of it, by placing our patients in an erect posture, we are enabled to effect by the loss of a few ounces of blood, what could not be obtained in a supine one by the loss of very many. This circumstance may also aid the timid practitioner, who may be afraid to venture on the great depletion that sometimes becomes necessary to produce this effect, when this situation is not attended to. I have therefore always placed my patients on their feet, when practicable, before I opened the vein. We may also spare ourselves a great deal of trouble, and our patients much anxiety, in cases of tedious labour from the unwillingness of the soft parts to dilate, where this disposition to syncope from the loss of a few ounces of blood when in an erect posture obtains, by taking advantage of this peculiarity. Few women object to being bled during labour, nay, they for the most part think it indispensable when the labour has been protracted. The consent, therefore, of the patient and her friends is easily obtained; all we have to do is, to conceal from them our wish to have her become faint. Should she faint we can readily satisfy them, by calling to their recollection, it is common to her from bleeding, and that it will be useful to her. In this way, we can very often shorten a labour many hours, when we might not be able to obtain the consent of our patient, or we ourselves think it necessary, to bleed her to the extent of forty or fifty ounces.

## CASE II.

ON the 26th September, 1800, I was called to the wife of Michael Falkrod at Frankford, in consultation with Dr. Ruan.\* She had been in labour twelve or fourteen hours, with her second child;† the pains frequent and strong; the waters discharged some time; the head was situated favourably, and completely occupied the vagina; the perineal tumour large; the os externum not larger than a common finger ring; admitting the finger with some difficulty in the absence of pain; during pain, thrown up against the inferior edge of the pubes in such a manner, as not to admit the finger or allow it to be retained if previously introduced. Externally, a strong cicatrix was found running to the very verge of the anus; internally, it could be traced further. This cicatrix prevented the unfolding of the external parts so effectually, that the repeated efforts of the uterus, for several hours, were insufficient to make them yield, though the head had been closely applied to them during that period.

This patient was a strong healthy woman; considerable fever had been excited; the pulse strong, frequent, and hard. I proposed bleeding *ad deliquium*, to which Dr. Ruan consented. We immediately opened a vein and took about forty ounces of blood; but her pains were so rapid, we were obliged to take it from her in a recumbent posture, and no disposition to syncope was manifested. This quantity, however, had some effect, as there was evidently a beginning relaxation, and an abatement of the violence and frequency of the pains. We now agreed upon a second bleeding, and to have it taken in an erect situation. We with some difficulty effected this, when,

\* It is but justice to state, that Dr. Ruan is not the physician alluded to in the note below.

† With the first, she had suffered an extensive laceration of the perineum. It is somewhat remarkable, that all the cases I have related, were in the same neighbourhood, and all had been under the care of the same physician.

upon taking five and twenty or thirty ounces more, she fainted ; she was laid upon the bed, and in a few minutes, by the forceps was delivered of a fine healthy boy. Our patient recovered rapidly, without accident or drawback.

### CASE III.

FEBRUARY 26th, 1803, I was called to the same woman in labour with her third child. The same circumstances attended, and the same remedy was employed, with a similar result. This case was witnessed by Mr. Bond, an ingenious young gentleman of Baltimore.



## A CASE

OF LONG CONTINUANCE OF PULSATION IN THE FUNIS UMBILICALIS AFTER THE BIRTH OF THE CHILD.

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ON the 12th of May, 1794, at 8 o'clock A. M. a lady was delivered of a female child. The child did not cry or breathe as is usual immediately after delivery, but remained motionless for three or four minutes, before any signs of respiration manifested themselves. I was surprised at this unusual delay, as neither the circumstances of the labour, nor the appearance of the child could account for it—the labour was quick and natural; the child though not very large was apparently healthy and well formed; the pulsations of the umbilical arteries brisk; in a word every thing was as ordinary except the want of respiration: to establish this, I inflated the child's lungs, after waiting the three or four minutes just mentioned, which in part succeeded as to its object; the child after this began to inspire slowly, the expiration was protracted and attended by a peculiar croaking kind of noise—respiration was for some time carried on in a slow and irregular manner—the child sometimes breathing pretty freely for a minute or two, and then appearing to be much oppressed and in pain—respiration would now be entirely suspended from ten to thirty seconds; the child during this time would writhe its little body as if much pained, its face would become livid and the pulsation in the funis would be much augmented; after this kind of struggling would subside, it would appear pale and lifeless as though it had fainted. In this manner did things go on until half past two o'clock in the afternoon, a period of six hours and a half—at which

time the umbilical arteries ceased to beat, and the child expired; the cord during the whole of this period was of course suffered to remain entire : the pulsation in the cord did not cease suddenly, as the forces of the arteries were observed gradually to diminish some minutes before they ceased to beat.

During all this period of six hours and a half, nothing untoward happened to my patient, she had neither flooding nor pain.

The state of the placenta was from time to time examined—it adhered to the inferior anterior part of the uterus—its lower edge could be felt above the pubes, and just within the mouth of the uterus.

There were no particular pains taken to prevent the funis being exposed to the air, or to maintain its warmth by any applications.

When I proceeded to deliver the placenta, I found it lying loose in the vagina, its extraction was effected without any difficulty, nor was it followed by any uncommon discharge of blood ; but pains succeeded.

Upon the history of this case I beg leave to make a few remarks : first, there being no hemorrhage in this case during the retention of the placenta proves, I think almost to demonstration, that it only happens from that part of the uterus to which the placenta is attached :—secondly, that the tonic\* contraction of the uterus may for a long time be suspended without danger, if the placenta continues its connection with the uterus :—thirdly, that the uterus was in a state of torpor or in-elasticity for the whole, or the greater part of the time ; for had its tonic action taken place, the placenta would have been thrown off, or at least the circulation in it would have been destroyed ; as we find even during some laborious labours, where both the tonic and spasmodic contractions,† after the evacuations of the

\* By tonic action or contraction, we mean that constant and regular action whereby the uterus reduces itself to its original state after having been distended.

† By spasmodic contraction, we mean that irregular action of the uterus that is excited by stimuli, and is commonly labour or after pains.

waters, are powerful, that the blood passes with such difficulty through the uterus, that the circulation in the placenta is with difficulty maintained ; and is, in some instances, entirely interrupted, to the destruction of the child, long before its delivery :—fourthly, that nothing could have been more fortunate than this uncommon attachment of the placenta in the case before us ; for had it been thrown off before the uterus had recovered from its state of torpor or in-elasticity, (as is sometimes the case,) a fatal hemorrhage might have succeeded ; for it is but by this tonic action that hemorrhage is prevented always following the detachment of the placenta—it is on this power, that the practice of Puzos is founded, of rupturing the membranes in a certain species of flooding cases, that the uterus may contract, and shut up the mouths of the bleeding vessels :—fifthly, her having no pains during the attachment of the placenta, shews that the spasmodic contraction is dependant on the tonic ; for where it not, we cannot conceive why it should have taken place, since the placenta (a sufficient source of irritation) remained to stimulate the uterus for more than six hours ; and the pains which succeeded the delivery of the placenta, were, consequently, after the tonic contraction had taken place :—sixthly, there having been no particular pains taken to prevent the exposure of the funis to the air, nor any warm application to maintain its heat, shews that the death of children, in cases where the funis is prolapsed into the vagina, or is without the labia, is not owing to the influence of the air stopping the circulation in it (as some have imagined,) but is in consequence of some pressure exerted on it at the margin of the pelvis or elsewhere, more especially as this must almost necessarily take place, in cases where prolapsus most readily happens, that is, in belly presentations :—seventhly, the gradual diminution that was observed, in the force of the umbilical arteries, for ten or fifteen minutes before they ceased to beat, marks, I conceive, the uterus recovering its tonic contraction ; for so soon as it began to pucker up, the circulating force, both in it and the placenta, was di-

minished; at length it contracted so much, as to entirely impede that through the placenta, and eventually throw it off. At what precise moment this happened I cannot determine, as about fifteen minutes were employed in exertions to save the child, and during which time, I did not touch; but suppose it took place at the time the circulation in the funis stopped, or very quickly after, for as I have observed before, when I went to deliver the placenta, I found it loose in the vagina.

How much longer the child might have lived, had not its connection with the mother been destroyed, I cannot pretend to determine, but think it probable, it might have continued, as long as that circulation may have been maintained. It appeared to me to have been entirely dependant on the mother for the support of its short life, for almost instantly after the pulsation in the funis stopped, it expired. And if we take a nearer view of the circumstances that attended this case, we shall not find the situation of the child after its delivery to be so very different as we at first sight might imagine—It was still connected to, and supported by the mother; it was not surrounded by the liquor amnii, but this is by no means directly essential to the child, as we find many children born alive after it had been evacuated many hours, days, or even weeks, agreeably to some.\* The only circumstance of difference that strikes us forcibly is, its having breathed; but, even this is not sufficient to do away the possibility of its continuing to live, as long as it could be nourished by the mother, since it failed producing its most common and important effect, namely, a change in the circulation of the blood.

Besides what has just been said, I think this case very naturally suggests the following queries. How was the circulation between mother and child maintained? Was it owing to any particular structure of the heart of the child? Was it owing to the foramen ovale remaining open unusually long, or in other words, to its having no disposition

\* La Motte, &c.



to close? Or was it owing to any mal-conformation of the lungs, preventing that free circulation through them that is so necessary to the establishment of life? Is this query rendered more probable, by the livid appearance of the face that occasionally took place? Should we not suppose, that a certain specific irritation is necessary to be excited in the lungs, either by the blood, the air, or by both, before the obliterating process (if I may be allowed the expression,) can take place at the foramen ovale? and that something more is necessary to produce this, than the mere expansion of the lungs? or does it only suppose that, when this want of disposition in the foramen ovale exists, the circulation through the lungs cannot properly be established?

## AN ATTEMPT

TO EXPLAIN WHY MORE CHILDREN LIVE THAT ARE BORN AT THE SEVENTH, THEN AT THE EIGHTH MONTH OF PREGNANCY.

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IT is an observation as old as Hippocrates, that children born at the eighth month of pregnancy, are not so viable as those born at the seventh. This opinion has almost universally obtained credence, without, as far as I know, any rational explanation being given of it. Hippocrates has attempted it by saying, that at seven months the child naturally makes an effort to deliver itself, in which if it failed, it again attempted at the eighth month, and should it succeed, it came into the world weak and infirm, from having exhausted itself by these efforts. This hypothesis is scarcely worthy of refutation, since, agreeably to this principle a child, at full time, ought to be still more enfeebled, as now it has made three attempts at escape. Nor is the opinion of Pythagoras, that the number eight is less fortunate than that of seven or nine, more rational or satisfactory.

Those who have controverted this point, have not been more fortunate than their opponents ; since they have rested the whole merit of their defence on a single circumstance of the child, (namely, because it has tarried a month longer in the womb, it must be more perfected, consequently more likely to live) without ever adverting to the contingencies of the uterus or labour at this period of gestation.

I therefore conceive this opinion of Hippocrates just, with this explanation ; that a child at eight months is not less viable than the one at seven ; but that its chance for

life is diminished, owing to particular circumstances influencing the uterus and birth of the child. For agreeably to observation, we find many seven months children reared, but very few of those of eight months. Let us inquire to what circumstances this may be owing. I believe them to be connected only with the uterus, and entirely independent of the child itself.

These circumstances are, first, to a greater power and disposition in the body and fundus of the uterus, to contract and throw off its contents, at the period of seven, than at that of the eighth month; secondly, on the neck of the uterus being at this period more powerfully constrained to relax or yield to these contractions.

That the body and fundus are more disposed, or more violently induced to contract at this time, I infer, from their being now stretched to the greatest limits they can bear with impunity; and when thus stretched are stimulated to contraction: this contraction is manifest to the touch if the finger be introduced through the *os tincæ*. They have now also a difficulty to overcome which is essential to the further progress of gestation, which is the resistance the neck makes to development, and it is well known the uterus contracts *cæteris paribus* in proportion to the obstacles which oppose it.

Until the seventh month, the body and fundus afford almost exclusively the necessary room for the continually increasing ovum;\* this happens from, First, the germ being deposited within their cavity, and consequently acting immediately on their fibres, which it distracts in all directions, but more especially in their longitudinal: Secondly, to these fibres being longer and more lax than those which compose the neck; the former, therefore, oppose but little to the ovum, whereas the latter do a great deal. This is a wise provision of nature, otherwise abortion would always take place.

But this disposition to distention has its limits;—and the resistance of the neck cannot be maintained beyond a

\* Baudelocque, Denman, &c.

certain period ; this period is when the body and fundus are thoroughly developed, which happens for the most part at the seventh month. These parts now refuse, or cannot, without injury or pain, be stretched any farther ; they will consequently contract from the stimulus of distention ; the influence of this contraction must be felt somewhere, and this where there is now the least resistance ; and this will be at the neck of the uterus, which will not only be passive, but open to a certain extent, and over the aperture of the pelvis ; while the body and fundus are not only in a state of action, but are defended and supported by the abdominal muscles, &c. This resistance and action of the upper parts of the uterus continuing, and their contents augmenting, constrains the neck to develope, as its resistance is only passive.\* By this arrangement the neck of the uterus, after the seventh month, is obliged to afford all the room necessary for the future increase of the fœtus, &c.

From the balance of power being now in favour of the body and fundus, and this being exerted on the neck, it follows, it must either yield, or the uterus must rupture ; but as the uterus never has been ruptured in this way, we have a right to infer, that there is a disposition in this part to expand when acted upon by them. Now, should this disposition be greater than ordinary, or, in other words, should the resistance be inferior to the action it has to counteract, the whole neck will be speedily developed and the fœtus will escape through it without difficulty or danger ; and that this disposition, or rather obligation to unfold is greater at this period than at any other, is rendered probable ; first, from not only the contractions of the body and fundus being stronger, but being more powerfully felt from their being new, and consequently the parts opposing them more influenced by them ; and secondly, from a partial distraction of their fibres having been effected by these efforts, the neck is irritated, and such is the economy of these parts, that when this happens the contractions of the fundus and body are more powerfully and more frequently



renewed from sympathy, so that the neck is indirectly concerned in its own distention.

That the power of the fundus and body is greater both positively and relatively at this period, than at any other, I conclude, first (positively) from their fibres being but lately stretched to their greatest extent, and of course their strength not diminished as it afterwards is by the long continuance of this state; and secondly, (relatively) from resistance being now given to the farther development of the fœtus, they are powerfully stimulated by its continual augmentation, and contract in proportion to the exciting cause.

We need not therefore be surprised that abortions frequently happen at this period; for the neck now is to furnish the whole of the required room for the increasing bulk of the ovum: it is therefore constrained, by the powers just mentioned, to unfold; which if it does in a just proportion, no evil will ensue; but should this not be the case, and it should yield too readily, premature birth will be the consequence. Therefore, should the neck of the uterus withstand the influence of the body and fundus after their first efforts are manifested, the presumption is, it will resist it to the full period of gestation.

Three causes co-operate to this end; first, the contractile power of the fundus and body will rather be weakened from long distention, and in some measure from their becoming accustomed to its stimulus; secondly, to this stimulus being rather diminished now, since the ovum does not increase as rapidly as formerly, and to more room being allowed for that increase by the augmentation of the neck; and thirdly, to the neck being now less passive; for as it is forced to develope, or to augment in width, it becomes incorporated with the body and acts with it, so that when contraction is excited, all the fibres act at one and the same time, and such is the nature of this contraction at the eighth month and at after periods, that it tends to shut the mouth of the uterus; and hence we see at this period very few spontaneous premature births.

For the reasons just assigned, there is no time of gesta-

tion at which the uterus yields so unwillingly as at the eighth month ; and this emphatically accounts for the observation, that eight months children do not so frequently live as those of seven ; for we find that at seven months the mouth of the uterus will yield to the internal agents sometimes very readily, consequently the child does not suffer the evils of a long and protracted labour, nor the influence of external agents : whereas, at the eighth month, it almost always requires the concurrence of external causes or violence to throw the uterus into contractions ; hence, children of this period seldom live, for they have not only to contend with all the violence that may be offered externally which is capable of producing uterine contraction, but also, with all the accidents that may arise from a severe and protracted labour, as well as to their being ushered into the world before their final uterine development : we need not wonder therefore so few survive these evils.

A great variety of instances might be adduced to prove the safety and frequency of abortions at the seventh month ; Van Sweiten mentions several : \* La Motte† also furnishes them ; he not only tells us of its happening more than once to the same woman, but also to her daughters, so that with them it appeared constitutional. I myself know a lady who has been delivered five different times at the seventh month or very little more, all of which children, (with one exception) are now living and healthy.

Mauriceau,‡ however, opposes this opinion ; he says children born at the seventh month very rarely live beyond fifteen days. To reconcile these different sentiments is perhaps not very difficult, as I conceive very much will depend on the opinion the accoucheur or midwife may have of their chance to live. For it can very readily be imagined that very little attention will be paid, where such strict duties are required, if it be pre-supposed those at-

\* Comment. § 1310.

† Traité des Accouch. liv. I. chap. 23. p. 122.

‡ Liv. II. chap. 2. p. 204 and 205.

tentions will be unavailing or thrown away ; while on the other hand, we can readily believe that many may survive, who have that care bestowed upon them their delicate frames require. Thus we see of how much importance a pre-conceived opinion may be, and what different results may follow from the same premises.

It would certainly be erring on the right side to suppose, that all children born alive after the fifth month may, by proper attention, be reared ; and that we should never remit in our exertions and care towards them, however unpromising our reward may be. We have instances upon record, of children at five months being raised to manhood and old age. Thus we are told that Fortunius Licetus\* was an abortion of the fifth month, in consequence of a severe fright his mother received from a storm : when he came into the world he was no bigger than the palm of one's hand. His father, a physician, cherished his little body with great care by means of a furnace heated to a proper temperature by artificial means, (after the Egyptian method of hatching eggs,) and also instructed the nurse in the proper mode of nourishing it. He had the good fortune to reap the fruit of his diligent solicitude and care, by his growing up to manhood, and becoming famous for many works of erudition ; he lived to nearly the age of four-score. Another instance may be given not less extraordinary, and of more recent occurrence.† “ A fœtus at five months of pregnancy was delivered alive, but puny and weak to excess ; it did not cry, and seemed hardly able to breathe ; its eyes were closed, the limbs flabby and relaxed ; some little motion, and the warmth of its body were the only signs of life it exhibited. It was wrapped up in soft linen, and supported by a proper degree of heat ; a little luke warm milk was given it drop by drop ; this it swallowed ; it continued precisely in this way for four months ; its motions were very inconsiderable, and it uttered no cry ; it voided no excrements. At the expiration

\* Baillie Jugem. des Scavants, tom. V. part. I. p. 239.

† Brouzet. Essai sur l'Education Medic. &c.

of the four months, it began to cry, to void excrements, to move its body, to suck, and to grow like other children ; so that at the end of sixteen months, it exceeded in size ordinary children of that age."

These instances prove to us how careful we ought to be in pronouncing weak fœtuses not viable. It would indeed be a good rule to treat all such children as though there was a certainty of their living ; nay, this appears an indispensable duty, since we have no right to withhold our attention, however tedious or fatiguing it may be, because many have died who were delivered at this period. The anxious solicitude of some parents has rescued many poor little unfortunates from an untimely grave : if then our affection prompts us to exertions of this kind, humanity and duty ought surely to have equal claim. I am at this time attending a little creature now a month old, that was born at little more than the sixth month. When it came into the world, the little noise it made did not amount to a cry, but it stirred its little limbs with considerable vigour, and after a few days was enabled to take sufficiently of whey sweetened with loaf sugar, to nourish its feeble frame ; it now sucks its mother frequently during the day and night, has regular evacuations, cries audibly, and promises fair to be reared without much more than ordinary trouble.

Thus, I have related instances of children living, that were born at the seventh month, and even at a much earlier period ; but am not able to give any one, from authors, that was born at the eighth month. What has occurred in my own practice, goes to support what I have already advanced, that children born at the eighth month, more rarely live than those born at the seventh.—I know but of three instances of children living that were born at the eighth month, and these three were from the same lady, who habitually was delivered at this period : her labours came on spontaneously, advanced favourably, and terminated speedily. All other cases of labour, *that I have seen at this period*, were preceded by more or less of external violence, or frights ; the uterus was therefore sur-



prised (if I may use the expression) into contraction, and therefore performed its duties irregularly and reluctantly. The os tincæ now not only yields less willingly, (from whatever cause) but the increased volume of the child offers more resistance to its passage ; the union of these causes protracts the labour to a considerable period, so that the child either perishes before it is born, or else is expelled so very much exhausted, that it survives its birth but a short time.

From what has been said, I think we may safely draw the following conclusions : First, that children born at the seventh month, have, from the contingencies mentioned, a better chance of life than those born at the eighth month. Secondly, that children born at the eighth month, though more viable, strictly speaking, yet from the circumstances of labour at that period, are more exposed to danger than those at the seventh, and hence their diminished chance of life. Thirdly, that of those children born at the eighth month and those of the seventh (*cæteris paribus*,) more of the former would live, than of the latter. Fourthly, but from the nature and physiology of the uterus itself, there ever will be more seven months children reared, than of the eighth.

CASE OF

**RUPTURED UTERUS.**

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ON the 18th September, 1796, at three o'clock in the morning, I was called to the wife of Anthony Serance. She had been taken in labour about six o'clock the preceding morning with her third child. The account I received on my arrival from the midwife, was, "that her labour began regularly and smartly; that the presentation was natural; the mouth of the uterus dilated kindly, and the waters had escaped about an hour after her arrival; that from appearances, she expected delivery would have been effected by 8 o'clock, (that is about two hours after its commencement;) in this she was disappointed; the pains continued with more or less violence all day, and the succeeding night; that towards morning, (twenty hours after the labour had begun) she screamed out from sudden pain about the left groin, and upon examination, per vaginam, the child could not be felt, though very easily touched before; a faintness succeeded, with severe puking of blackish matter; violent palpitations of the heart, great anxiety and restlessness. About an hour after these events had shewn themselves, I was sent for. I found her in the condition just mentioned, together with cold extremities, profuse sweats, laborious respiration, and an almost imperceptible pulse.

From the symptoms present, and the account received from the midwife, I immediately suspected the nature of my patient's disease; and before I examined her, mentioned my opinion, and also the most probable consequence. I

now proceeded to satisfy myself, and found my conjecture realised. The child with the placenta had escaped from the uterus, through a rupture at its left anterior part, just at its junction with the vagina, which was at this time sufficiently large to admit of the passage of my hand without the least difficulty; the uterus was situated just above the projection of the sacrum, and was contracted very firmly, and of the size it usually is at this period, after it has parted with its contents. The child and secundines were found lying in the cavity of the abdomen among the intestines; and never shall I forget the horror with which I was seized when I found my hand among them.

I took hold of the child by the feet, and delivered it immediately without the smallest difficulty; this I considered the only alternative; and this from the time that had elapsed, as well as from the nature of the accident, was less than a forlorn hope. The placenta was with equal ease withdrawn.

The puking with the other distressing symptoms still continued; she complained of excessive thirst; but the liquids she drank were incessantly rejected; the pulse now was so extremely frequent as not to be numbered, and so small as scarcely to be felt. Her distress and anxiety were great beyond conception; calling upon all around, either to help or to kill her. In this situation she continued until about seven o'clock, twenty-five hours from the commencement of her labour, and about five after the rupture of the uterus.

Leave was obtained to inspect the body, and my friend Dr. Physick very obligingly assisted me in it. On opening the abdomen, several quarts of very dark bloody fluid were found in it; part of which was doubtless the blood that escaped from the lacerated parts, and the rest perhaps was an increased quantity of the fluid natural to this part, or portions of the peritoneum relieving themselves by effusion; together with a part of the liquor amnii, as we cannot suppose all escaped at the rupturing of the membranes. The stomach and intestines bore marks of high inflamma-

tion, and in many places approached gangrene. The whole lining of the abdomen partook of the same appearances.

The pelvis on examination was found faulty at the superior part, by a projection of bone or an exostosis ; it was situated a little to the left of the symphysis pubes, and looked towards the base of the sacrum ; it diminished the superior cavity of the pelvis in the direction of its small diameter about half an inch ; it was sharp, and pointed at its extremity.

The uterus was firmly contracted ; its body and fundus participated in inflammation with the abdominal contents, while its neck, or that portion which constitutes neck when not impregnated, was in a state of gangrene, or perhaps sphacelus, excepting a small portion of the posterior part. The connection between the uterus and vagina was destroyed nearly two-thirds round ; the laceration extended into the body of the uterus about an inch in its contracted state.

The projection of the bony process just mentioned, by diminishing the cavity of the pelvis, must be regarded as the remote cause of this accident. The child's head enveloped in a portion of the uterus rested against this point so long, that inflammation and gangrene were the consequence. The efforts of the uterus continuing violent ; and perhaps that violence increased by the difficulties which opposed them, eventually gave way, as its substance at this particular part was much weakened by the changes produced on it by inflammation ; nay, the bony tumour acted somewhat like a cutting instrument. This unfortunate woman had been twice delivered by the crotchet, which, from the nature of the deformity, was not necessary ; and from the time that was allowed to elapse, not warranted on any principle of sound practice ; as there was every reason to believe the children were alive when it was employed.

The nature of this deformity must have readily been discovered, had an examination been made of the superior strait, for it was immediately perceived when I introduced my finger into the vagina : nor did it exist in such an ex-



treme as to indicate the crotchet, as there was still near four inches at this part of the pelvis, and consequently the delivery was not retarded by its smallness, but by the bad direction it gave the head ; that is, obliging it to present too transversely at the superior strait, by which means one of the parietal bones, and that near its protuberance, must have rested on this bony edge, which of course would prevent its descent.

Had this woman's children been small, she might have been delivered naturally ; or had the head presented with its vertex towards the right acetabulum, its direction with respect to the superior strait would have been so diagonal as readily to have passed this projection. I cannot however undertake positively to say the head did not in any one instance present in this way, but presume it did not, as the crotchet was employed in the former labours, and a rupture of the uterus was the consequence of the last ; besides, this presentation is by no means as frequent as the one where the vertex is to the left acetabulum, not occurring once where the other does ten times.

Be this as it may, her children most probably, and herself most certainly, might have been saved by turning, for not the least difficulty was found in making the head pass the superior strait when I delivered the child from the abdomen ; a proof of sufficient room was the head made to engage properly, or head reversed, for when we deliver by the feet, the head engages like a wedge, from a smaller to a larger diameter, and will pass very readily in this direction when considerable difficulty might be experienced from the other ; another advantage to be derived from this change of situation of the child, is, that we can constantly make the head obey a diagonal, or a completely transverse direction, either of which would, I trust, have saved the children and mother. In a pelvis of this kind I conclude the exclusive indication is turning.

I did not hesitate a moment to deliver my patient after I discovered her situation, though I proposed no success to her friends ; death I thought inevitable in either case ; from

delivery there was a possible chance for life, from neglecting it none. The propriety of this reasoning, however, has been called in question, as it is conceived any attempts to save the patient, is but to prolong her misery.

No point of obstetrical practice is more unsettled than this ; but why it is so, is difficult to say, since it may become obedient to rule, without the smallest difficulty, or involving in it the slightest doubt or contradiction.

We have three principal directions on this subject : by the first we are told it is improper to do any thing, since it is conceived no effort can be availing ; the patient is here allowed to expire without an endeavour to save her ; of this class were the respectable Dr. W. Hunter, Dr. Denman, and some others of less celebrity. Dr. Douglass, in a treatise on this subject, has combatted this mischievous supineness so successfully, that it would be idle to say any thing more on this subject.

By the second, we are recommended to deliver as expeditiously as possible through the pelvis ; this practice is sanctioned by more numerous, and not less respectable names than the first, among whom we may mention La Motte, Levret, &c. This plan has a decided preference over the other, since it offers assistance, and some women have escaped from death by it.

By the third, we are taught to believe the woman has no resource but in the cæsarean section : this plan, agreeably to Baudelocque, was originally suggested by the ingenious but timid Levret, but in terms so indirect, as clearly shows both his knowledge of the subject, and the prejudices it would have to encounter.

Having thus briefly stated the various opinions of authors on this subject, I shall take the liberty to say, that either is wrong if implicitly followed ; and to be right upon this subject requires the adoption of all three, as circumstances may require. It must however be remarked, that the first can never be followed without incurring the imputation of blameable timidity, except where the patient is

absolutely in articulo mortis; here we may withhold aid, as nothing can benefit the expiring sufferer.

The second plan cannot be implicitly followed; for instances have occurred where the rupture was through the substance of the uterus; and as soon as the child has escaped either entirely or partially into the abdomen, the aperture is so much diminished by the contraction of the uterus as to render it impossible to deliver it through the accidental and natural passage. But, when the rupture happens, about the junction of the uterus with the vagina, which is by far the most frequent, and at the same time is not subject to the diminution of size just spoken of; the second mode is exclusively indicated, unless such deformity of pelvis is connected with it as would render the passage of the child through it impossible.

The third plan, I conceive, is only admissible, first, where the rupture has happened to the body of the uterus, and delivery through the vicarious passage rendered impossible by its contraction. Secondly, where there is complicated with this accident, such deformity of the pelvis, as at once to forbid any attempt at delivery through it.\*

\* See Essay upon the Rupture of the Uterus.

## REPLY

TO DR. PEACHEY HARRISON'S OBSERVATIONS ON

## IMPREGNATION.

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I AM extremely averse to enter on any thing like controversy ; but am obliged to notice " Observations on Impregnation, by Dr. Peachey Harrison," as, the author of them seems satisfied that he has removed the obscurity that has so long veiled this interesting process. This, in my opinion, he has not succeeded in,\* notwithstanding he has pursued the "coy maid" (Nature) into some of her recesses, and endeavoured to extort from her some of her oracles. I boast of no such familiarity with this high personage, and shall be very well satisfied if ever she allows me a glimpse of her operations, either through key hole or cranny. I freely acknowledge she has never admitted me into her recesses ; I will not therefore pretend with certainty to say, how she performs her secret actions within them.

Did we implicitly rely on this gentleman's assertions, we should have nothing to wish for on this subject, as he assures us his opinions are not "deduced from uncertain conjectures, but from responses, rendered by interrogated nature." But alas ! this same "coy maid," when he pursued her into her secret recesses, appears to have played him a slippery trick ; for, instead of "the responses of interrogated nature," she appears to have furnished him

\* As he has not brought forward a single substantiated fact, a probable conjecture, or a plausible hypothesis.



with nothing but "uncertain conjectures:" this was perhaps intended as a punishment for pursuing "her into her secret recesses," and endeavouring to wring from her some of her oracles. He confessedly admits her responses were extorted, they must therefore have been given unwillingly; and that which is given by "compulsion" (even where reasons are as plenty as blackberries,) is not always given truly. But can that which is given by Nature be otherwise than true? No; but is the gentleman certain that it was from Nature's oracle the responses came?

Let us endeavour to ascertain this; he tells us, "That there is a specific sensibility resident in the female genital system;" and that "this sensibility resides in the os tincæ chiefly, but also in the uterus itself, in the fallopian tubes, and *perhaps*, in the ovaria." Is this the responses of nature or conjecture? Part is certainly admitted as such, since he says, "*perhaps*, in the ovaria;" this then is certainly not the dictum of the "coy maid." But let us proceed. "I am inclined to think, (mark, inclined to think,) the ovaria possess a share of venereal sensibility; because, as venereal desire is first awakened in males by the stimulus of the semen, *so it is probable*, (very candid from a man whose opinions are not deduced from "uncertain conjectures,") it is first aroused in females by one or more ovula acquiring maturity." Are these facts? No; it is well known that venereal desire is awakened in males before the secretion of semen, and even after the extirpation of the testes. Boys at very early periods give evidence of passion of this kind, and before we can possibly suppose semen to have been secreted, unless this secretion takes place at a period not dreamed of by physiologists. Who has not witnessed the most flagitiously wanton acts in boys of ten years old or even younger? I myself have lately witnessed an act of this kind, the most horrible to be conceived of; a boy of nine years old, was caught in the act for venereal gratification with a child of eighteen months, the child was under my care, and was much injured. But this is by no means a solitary case; every boarding school

for boys, will furnish proofs of early desire. After the extirpation of the testes we know desire remains; the eunuch, the horse, and barrow furnish instances of it. In the female nearly the same observations will apply; girls are known to exhibit many marks of libidinous desire, before we can with propriety suppose "one or more ovula" have acquired maturity; they certainly do not have these desires where we know "one or more ovula" are perfected; and women retain their venereal appetite, when we must suppose there is not "one or more ovula" to be matured; namely, after the menses have ceased to flow; and we may add, there are women of very warm passions, and who perhaps have enjoyment from sexual intercourse in its most exalted form, yet ever remain barren. Where are the ripened ovula in such women, or how are we to account for their receiving gratification, when we have no right to suppose, "one or more ovula" have acquired perfection to produce the venereal impulse?

"That the venereal sensibility resides in the *os tincæ*, &c." continues Dr. Harrison, "I infer from the structure of the parts, and especially from the projection of the *column uteri* into the vagina, where it will of necessity, in the sexual intercourse, receive irritation from the soft and velvet-like head of the penis, well calculated to produce what I have called the venereal orgasm; which consists in a certain excitement of the uterine system, accompanied with exquisite sensations, similar to those which take place in the male, at the time of the ejection of the semen, and, with an *unknown desire* to receive the venereal stimulus."

From this it would appear that the *os tincæ*, in consequence of the irritation from the male penis, becomes the active source of venereal pleasure. How does this tally with fact and observation? We will inquire. The *os tincæ*, from the firmness of its texture, and general want of sensibility, as far as can be determined by the touch, seems but ill calculated for this important office, more especially as all women, as far as we know, have this part;

but there are many women who feel no pleasure from this act, and others to whom the venereal congress is truly disgusting, and yet these women are prolific. Now unless it can be proven, that the *os tincæ* of the woman who feels no pleasure, and that of the one who does, be different, we must conclude, that the *os tincæ* is not the seat of venereal pleasure. It perhaps may be doubted by those who have not made the inquiry, whether this circumstance obtains: but for the fact I can pledge myself, at least, as far as the testimony of the women themselves may go: inquiries were made of many with the view of ascertaining the fact; and they were conducted under such circumstances as could leave no room for doubt; the questions to this point were always answered with seeming candor, because they were thought essential to the history of their complaints, or necessary to their cure. Besides, women in the latter months of pregnancy, who do feel pleasure from sexual intercourse, have equal enjoyment when the *os tincæ* is entirely obliterated, or out of the reach of the penis.\*

We should be glad to know, what is to be understood by the female feeling an unknown desire; can we have a desire and not know it? If this be not an Irishism, it is very much like one.

"Irritation applied by the *glans penis* to the *os tincæ*," says the Doctor, page 423, "well prepared to receive this irritation, by its projection into the vagina, and by its extreme sensibility, is the immediate cause of the venereal orgasm, or that *unknown desire* which the female sometimes feels, in coition, for the seminal stimulus; and se-

\* The fact is of high import, and Dr. H. appears to have been aware of its force, since he endeavours to obviate it, by calling in question, in some measure, the fact, by saying there is "a diminished venereal sensibility," and a retraction of the *cervix uteri* from the vagina in the progress of gestation; and hence it seems highly improbable that, during this period, the venereal orgasm never takes place.

If we understand what Dr. H. means by venereal orgasm, we shall certainly insist, that the gratification from sexual intercourse (in women who ordinarily enjoy it) is as perfect in the ninth month of gestation, as in the first, or before it has even taken place.

condly, that an absorption of the fecundating fluid, when applied to the os tincæ during the orgasm, is the proximate effect thereof; and in the third place, that the transmission of the semen to the ovaria, by the proper action of the uterine system, is as naturally the consequence of absorption, as deglutition is of agreeable aliment taken into the pharynx. And lastly, that impregnation is the final cause of the sexual intercourse, and of the pleasure with which it is accompanied."

"It clearly follows from what has been stated above, that impregnation will never take place unless the venereal orgasm has been excited; and it is equally plain, that it will not happen unless the semen is brought in contact with the os tincæ during the venereal orgasm; and it follows with equal clearness, that an apposition of the urethra to the os tincæ, at the same time will be important, if not indispensable; not indeed for the purpose of the injection of the semen into the uterus, but that the seminal stimulus be applied to the os tincæ at the moment when it is prepared to absorb it and convey it to the ovaria."

It has already been observed, that in our opinion the os tincæ has little or no sensibility; or certainly not that degree of it as would render it the seat of pleasure, were it ever so well situated to receive the reiterated frictions of the penis; but it is by no means well situated for this purpose, as it is subject to a great variety of casualties which will prevent its having a fixed place within the pelvis. It therefore cannot from its "extreme sensibility" be the immediate cause of the venereal orgasm, or "*that unknown desire* which the female sometimes feels in coition, for the seminal stimulus;" first, because it does not possess the sensibility which the author of "Observations, &c." would attach to it; and secondly, that if even it did possess it, its uncertain situation within the vagina, would render it extremely difficult to profit by it. It is, we conceive, altogether an assumed principle, to make the seat of venereal pleasure resident in the os tincæ; it is one that cannot be proven either by analogy or experiment;



for, on the contrary, this part has been touched an hundred and an hundred times by every practitioner of midwifery, without their having ever discovered it to possess "extreme sensibility."

But did it possess sensibility even to the extent the gentleman would wish, how is its form calculated to call it into action? In the un-impregnated state of the uterus, but more especially in virgins, this part is conical, with its apex looking outwards; its extreme part is drawn almost to a point; at its sides spaces are offered by its being pendulous in the vagina; the penis may have with the *os tincae*, (supposing it fixed,) five places of contact; above, below, on each side, and directly against its point; now should "the velvet-like head of the penis" come immediately in contact with this extremity of the neck of the uterus, (which Dr. Harrison insists on being necessary that it may receive the seminal stimulus) what would be the consequence? first, the penis would receive considerable injury from coming frequently against so firm a body as the *os tincae*; and secondly, it would be productive of great pain to the woman, as happens when this contact does take place, in cases of any degree almost, of precipitation of the uterus, by rudely forcing it upwards by the projection of the penis: pleasure therefore cannot result from the extremity of the penis coming in contact with the extremity of the neck of the womb; if pleasure then must be produced by these bodies touching each other, it must be by the penis passing from side to side, or from top to bottom of the neck, much after the manner a dexterous butcher whets his knife on a steel. But this mode of producing pleasure, or the venereal orgasm will not answer Dr. H's. purpose, since the semen may be thrown on either side of the *os tincae*, and of course would fail producing impregnation. "Had this fact and some others" we shall presently state, been known to Dr. H. he would not have adopted the hypothesis which he labours so strenuously to maintain.

By venereal orgasm, we must understand a something

produced by, or the consequence of pleasure ; and this effect is conceived absolutely necessary to impregnation : we have already remarked, and shall now insist, that this is by no means necessary. Our reasons for thinking so are, first, That many women are perfectly indifferent to the venereal congress ; some affirm they never felt any thing like pleasure, and others that it is not only disgusting, but extremely painful : secondly, many women who labour under procidentia uteri have conceived, where the situation of this viscus, (their own acknowledgments out of the question) forbids the idea of pleasure resulting from sexual intercourse. But we think we have a right to insist on the weight, which their testimony gives us ; and this testimony goes directly to prove, that pleasure is not necessary ; nay, further, that conception has taken place in many instances of prolapsed uteri, where pain never failed to attend sexual intercourse. Thirdly, that, with other animals, such as the dog, frog, newt, &c. on which the ingenious and accurate Spallanzani experimented, pleasure or venereal contact was not necessary ; his syringes were sufficient, and they could hardly have been productive of pleasure ; the same we believe would obtain in woman. Fourthly, many instances have occurred where the occlusion of the vagina has been such, as to prevent most effectually the “ velvet-like head of the penis,” and the exquisitely sensible os tincæ from coming in contact. Dr. H. has thought to provide against these obstinate, and for him unfortunate facts, by saying, “ that an apposition may take place, appears to me entirely probable, (more probabilities ! where are the responses of the “ coy maid” to these important interrogatories ?) from the phenomena, which very generally occur in a sexual intercourse, in which the venereal orgasm is awakened ; among which are the following : the cervix uteri becomes turgid (how do you know this ?) the ligamenta rotunda contract ;” (what proof have you of this ?) have you not here asserted more than can be proven ? Could this be proven, it unfortunately would very much militate against your hypothesis, since the effect of this contraction

would be to make the fundus uteri approximate the pubes, and consequently make the os tincæ recede, or touch the sacrum; this situation of the os tincæ would be very unfortunate for the production of venereal orgasm, as it would be out of the way of the "velvet-like head of the penis."

"The uterus is depressed in the pelvis, and the os tincæ is brought nearer the orificium externum, and *assumes* a direction favourable to an apposition;" how can it "*assume* a direction favourable to an apposition," in cases like those recorded by Ruysch, Mauriceau, Hildanus, Harvey, &c.\* where not only one, but more barriers opposed the introduction of the penis into the vagina? in these cases we must regard the women who laboured under these disadvantages as virgins in effect, and we can hardly suppose the uterus could have been so extremely accommodating as to crawl down into the pelvis and apply the os tincæ directly to the small aperture that was free in these cases, that it might open-mouthed receive the ejected semen; for here it must be remembered that no venereal orgasm had taken place, since the "velvet-like head of the penis" was on the outside of the grating, and could only peep through a very small cranny at the os tincæ, anxious for "an unknown desire," but which, most lamentable to tell, could not be excited, because there was a most obstinate and envious partition between them. I would now candidly ask this gentleman, if he can for a moment suppose, the uterus shall so far change its situation, in virgins more especially, (for these women were physically so) as to bring the os tincæ down to the os externum, or even within an inch of it, where no disease of the vagina, as far as we know existed, to produce a procidentia uteri? I am sure he must answer,

\* Dr. Chapman and myself were invited by the late Dr. Clever to witness a case of this kind. The labour had been in full force for several hours, and the head of the child pressed strongly against the perinæum, but could not advance farther as the os externum was entirely closed by a dense membrane, in the centre of which there was a hole about as large as the barrel of a common quill, this was dilated by an incision, and the labour was terminated in three hours afterwards. In this case the husband acknowledged to Dr. Clever that he never had penetrated into the vagina.

he cannot. These cases will therefore furnish most unequivocally the following conclusions: first, that it is not necessary to impregnation for the penis and os tincae to come in contact. Secondly, that the venereal orgasm, as far as is dependant on this circumstance, is not necessary to impregnation; and thirdly, that Dr. H's hypothesis, which is entirely predicated on these contingencies, must necessarily fall to the ground.

Having thus noticed Dr. H's theory of impregnation, we shall proceed to consider his conjectures respecting superfœtation.

After having expressed some doubts as to the possibility of superfœtation taking place, Dr. H. observes: "But suppose superfœtation may happen, we are then only prepared to offer some conjectures as to the manner in which it may happen: and we would ask, why it may not take place as we have endeavoured to evince other impregnations do? This, it will be objected, will be impossible, the passage from the vagina into the uterus being obliterated after impregnation; but does it appear that this is the fact? Does not the smallest drop of blood effused into the uterus during gestation easily find its way into the vagina? The fact is, the passage is only obstructed with a viscid mucus, which might, it is conceived, be diluted in time of coition, so as to admit the passage of the semen into the uterus, whence it may be conveyed to the ovaria by the fallopian tubes, as in other fecundations; for it will not be asserted, that the openings of these tubes in the uterus are always obstructed by the placenta."

I have chosen to give a complete quotation of Dr. H's theory, that no charge of misrepresentation might attach; by this opinion it is admitted, that a viscid mucus does obstruct the neck of the uterus, and that unless this be diluted, the semen could not have access to the inside of the uterus—how is the semen to get mixed with this mucus to dilute it? Does it possess any chymical power over it? If it does not, how will the thick tenacious fluid, the semen, be made to act upon the viscid mucus already



present in the neck of the uterus, since it will have to overcome the attraction of gravitation to remain an instant in contact with it ! It may be said, that the attraction of cohesion is superior to that of gravitation ; let it be admitted ; the semen then can only act on that part of the mucus which is most inferior, and if it dilutes this, it will escape into the vagina, unless Dr. H. will point out some power in the uterus by which this inconvenience may be remedied. But we will not stick at trifles ; we will allow the mucus to be diluted by the semen ; but to effect this, it must be mixed with it, therefore the mucus and semen must travel together. What is there to obstruct their journey ? First, these fluids must separate the decidua from the uterus that they may pass ; how are they to do it ? Secondly, when they have effected this separation, they must continue it until they arrive at the fallopian tubes ; when they get there they must clear the decidua from their mouths ; how are they to do this ? chemically, mechanically, or by a species of legerdemain ? This Dr. H. is bound to answer and not us.

We have now supposed that these fluids have the power of separating the decidua from the uterus, with which it is in very intimate contact ; but will no injury result to the uterus from this separation ? Certainly, more or less hemorrhage will ensue at the instant of separation, and will flow into the vagina, for Dr. H. admits, that the " smallest quantity of blood effused," will do this ; there will then be counter-currents, the semen, &c. passing up, and the effused blood passing downwards ; where resides the power to give these different directions ? But it may be urged by the friends of this hypothesis, although Dr. H. has forgotten to avail himself of it, that the decidua is porous and spongy, and that the semen, &c. will be transported by capillary attraction—this to be sure is plausible, and might, as a dernier resource, be resorted to, had not Dr. H. himself unfortunately destroyed all claim to the subterfuge, by admitting that the smallest drop of blood effused into the uterus during gestation finds its way easily into the

vagina. Where is the capillary power when this is allowed to take place? or will Dr. H. suppose, there is a peculiar and "unknown" kind of attraction, between the "mucus diluted by semen," and the decidua? Be this as it may, it appears to us not only as matter of great improbability, but of total impossibility, that the semen should pass in this way to the ovaries. There is then but one more way left for the semen to get to the ovaria through the uterus; and this supposes the animalcular discoveries of Leuwenhoek to be established beyond doubt. We will suppose, then, in consequence of the union of the semen with the "viscid mucus" with which the neck of the uterus is obstructed, the latter becomes diluted; this is again supposed to be by single elective attraction; that is, the mucus in which the animalculæ are suspended, unites with the "viscid mucus" of the os tincæ, and becomes diluted, and sets them at liberty: they, as soon as freed from their bondage, by a species of instinct peculiar to themselves, scamper through the meshes of the decidua, travel through the fallopian tubes, and seek refuge and protection in the ovaria. I have thus offered a conjecture, at least as plausible as Dr. H's, to help him out of his difficulty; if he will not support it, it is not my fault.

## OBSERVATIONS

ON PARTS OF BURNS' HISTORY OF THE

### GRAVID UTERUS.

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THERE are few things less perfectly understood than the anatomy of the human ovum before the third month of pregnancy. We must also include in this charge, the condition of the uterus itself, and of its appendages, until this period of gestation : this has arisen from two circumstances chiefly ; namely, first, the infrequent occurrence of subjects proper for the investigation ; and secondly, the minuteness of some of the parts, or their very gradual, or very sudden change from their original or unimpregnated state. Hence has arisen the variety of descriptions given us by different authors, who saw rather what they wished to see, than what absolutely existed. Imagination ought never to supply what alone should be demonstrated ; it should never give its chimeras for anatomical facts. In matters of mere opinion, speculation is allowable ; but if indulged in to supply anatomical deficiency, it should be at least premised that conjecture is taking the lead for want of sufficient observation to supply the place of physical truths.

I have been led to these reflections from a work on the gravid uterus\* having just come into my hands, in which I conceive there is indulged no inconsiderable license of the kind just mentioned. This I truly lament, as there is considerable industry, observation, and acumen displayed in many parts of it. The errors, as I conceive

\* The Anatomy of the Gravid Uterus, &c. by John Burns.

them to be, I have no doubt will be corrected by the subsequent observations and reflections of the author himself, as they must sooner or later strike him, as circumstances not supported by anatomy, analogy, or reason.

The first circumstance I shall notice will be his account of the manner in which the ovum descends into the uterus, and the condition of the parts through which it passes. He says, p. 150, that "Before the embryo passes down through the fallopian tube into the uterus, that organ is every where lined with a vascular substance, which is produced by the action of gestation taking place in the uterus. This, which has received the name of the tunica decidua, consists of two layers, the inner of which is entire, but the outer is perforated at the os uteri and entrance of the tubes. This outer layer enters, for about an inch, within the fallopian tubes, and descends down the sides of the cervix uteri to its mouth, terminating in that gluten which shuts it up."

"The ovum is likewise covered (p. 151,) with a vascular coat, consisting of shaggy vessels, arising from the chorion."

I would ask, what proof is there that the "outer layer of the decidua enters, for about an inch, within the fallopian tubes?" is there any? is this a fact warranted either by dissection or analogy? I think it is not. My reasons for thinking so are, first, that it never has been demonstrated; and secondly, that it would be injurious. It would be injurious, by stopping up the mouth of the fallopian tubes, and thus offer an obstruction to the passage of the ovum; for we are not told that these orifices are increased in size during pregnancy, and if they be not increased in size at this time, it would be in vain that the anatomist should apply his knife to demonstrate an efflorescence of vessels for an inch within their cavities, since in their natural state they scarcely allow a bristle to pass. With what chance of success, then, can we hope that this will ever be proved by anatomy, more especially as all reason is against it? It will be idle to urge that, at this

*There are shaggy vessels in the decidua  
in the fallopian tubes.*



period of pregnancy they acquire more size, unless it can be demonstrated ; and the more so, as it would seem they dilated but to be obstructed. But let us ask for what purpose is the decidua within the tubes ; can any use be found for it ? The author himself urges none. May we not safely say, it would be injurious, and offer a great risk to the ovum being detained, and developed there, since the ovum itself is covered with a vascular coat " which is to inosculate with the decidua reflexa after it enters the uterus ? " And if it be covered with " a coat consisting of shaggy vessels " for the purpose just mentioned, why should this union not take place with the vessels of the decidua within the tubes, since it is admitted by all the experimental physiologists that the ovum or ova may remain some days in the tube. At least this has obtained in the inferior animals, and if analogy be admissible in an inquiry of this kind, we could have no hesitation to conclude, it would remain equally long with the human female ; and which no doubt would be time sufficient to form a connection every way firm enough to detain it within the tube ; the author himself says, p. 151, " that when the ovum descends into the uterus, it does not fall freely into the cavity, but is every where surrounded with a vascular coat from the uterus. With this coat the vessels of the chorion unite ; and were we, therefore, at this period," (that is, of its descent into the uterus) " to examine the ovum, we should find, that its shaggy vessels united, at one part with the decidua, at the fallopian tubes" (why, therefore, not within them) "and, at every other part, with the inner layer, which is pushed before it."

From this it would appear that, agreeably to the author, the union between the ovum and the uterus, or at least the production of the uterus, takes place very rapidly ; if this be admitted, it should also be allowed, that a similar union might take place within the tubes, unless some good reason can be given for the ovum's not inosculating with the decidua, as readily as with the decidua reflexa ; but it does

unite as readily with the one as with the other, according to his own doctrine, for he tells us, p. 153, that when the ovum falls into the uterus in a certain direction that it then unites with the inner layer of the decidua, which afterwards becomes decidua reflexa.

The next circumstance I shall notice is the mode in which the ovum is nourished, and descends into the uterus. "The embryo," says he, p. 152, "is at first a small speck, *growing* close to the sides of its membranes." We would ask what is precisely meant by the embryo growing to its membranes? Have they ever been separate? If they have, by what kind of union are they joined? And where does this union take place? The embryo, p. 152, is said most likely to derive its nourishment from "that portion of the shaggy chorion which covers the part of the membranes to which it is attached." It must be remembered that the ovum is now set free from the ovarium, and consequently not attached any where until it descends into the uterus to form an union with the reflected decidua; but the embryo must have support until this junction is formed; from whence does it derive it? From the spongy chorion; from whence does the spongy chorion derive it? This the author does not tell us. The chorion either has received a stock from the ovarium before its departure (which has never been proven,) or the extremities of its vessels must have the power of absorption, a circumstance wanting proof; and the more especially as we are told this shaggy coat is to be the bond of union between it and the decidua reflexa. For we are immediately after told, "when it descends into the uterus, the decidua gives an additional covering, and joins its aid to the increased demands of the embryo; and the two vascular coats form the placenta." From our author's account, therefore, I am much at a loss to account for the support of the embryo while it is at the mercy of the fallopian tubes, and before it forms a connection with its mother. It is true Mr. Burns, p. 115, seems to think he does away every difficulty by saying, "it is probable, that, at first, the embryo grows

by a kind of hydatid life ;” but does this explain it ? Is it not as difficult to form an idea of hydatid life as any other ? If the system by which this kind of life (if we may so term it,) is maintained, be less complex, is it less surprising, or of less difficult solution ?

I shall now advert to another speculation of our author ; here, some indulgence may be given safely to fancy ; but it ought to be admitted as a conjecture, and not advanced as a physiological truth. He says, p. 153, “ as that part of the membranes to which the embryo is attached, generally enters the uterus last, it follows, if this account of the formation of the placenta be true, (that is, of its being the joint produce of the spongy chorion and decidua reflexa,) that the placenta will be formed originally over that part of the uterus, where the tube enters the decidua, at that spot joining with the chorion to form it. But in some instances the case is reversed, and the embryo enters foremost, the rest of the membranes following it. When this happens, then the inner layer of the decidua, which was stretched across the orifices of the tubes, and which is afterwards to become the decidua reflexa, will contribute to the formation of the placenta.”\*

The author has indulged here in no common share of conjecture, which by the peculiarity of language gives a very imposing appearance, and which might be mistaken for a well ascertained fact. The manner in which the ovum enters the uterus must from the very nature of things ever remain in profound obscurity. We need never hope for this point to be cleared up, as the minuteness of the ovum itself ; the impossibility of ever comparing the manner in which they fall into the uterus ; but above all, the improbability that it should be witnessed in the act of falling, will ever place this matter among the many, which belong to conception, beyond the ken of man.

It is confessedly lawful on this part of the subject to indulge in hypothesis ; but it is misleading too far, when a

\* Leroux, long before Mr. B. indulged in very similar speculations.

language is employed that would convey the idea of the fact being thoroughly ascertained ; who would but believe that the mode in which the ovum enters the uterus, was well substantiated, when he reads, "as that part of the membranes of the ovum to which the embryo is attached, generally enters the uterus last ;" p. 153, and again in the same paragraph ; "but in some instances the case is reversed, and the embryo enters first." Who, I say, from this language would suppose, but what the author had frequently witnessed the fact, or has had some very cogent reasons for believing, that it entered precisely as he has prescribed ? The language of doubt is not employed ; it is given as a fact of which there could be no dispute. But we hesitate not to say, no one has ever had an opportunity to ascertain this point ; therefore, all that is said respecting the situation of the placenta must be received as conjecture.

It has ever been a puzzle to determine why the placenta should not always be attached nearly to the same place of the uterus. To do away this difficulty, Mr. B. has asserted, that the part of the membranes, to which the ovum is attached, generally enters the uterus last, having before premised, that the placenta is the joint production of the decidua and chorion, and that their union takes place opposite the part to which the fœtus is attached. It was therefore necessary to this theory, to suppose, "the part of the membranes to which the embryo is attached," should "generally enter last." And to account for the placenta being occasionally placed over or near the mouth of the uterus, it was also necessary to admit that, "in some instances the case is reversed."

I have already said, that there was no hope of this ever being demonstrated ; let us inquire now, how far it can be supported by facts and reason. From the premises laid down by Mr. B—— it must from his own confession follow, "that the placenta (p. 153) will be formed originally over that part of the uterus, where the tube enters the decidua ; at that spot joining with the chorion to form it." Now if this be admitted as a fact, we should always find



the placenta covering one of the tubes, or in other words that it would always be at or near the fundus of the uterus, except in those cases where matters are reversed, or where the embryo enters the uterus first. "When this happens," says our author, "then the inner layer of the decidua, which was stretched across the orifice of the tube, and which is afterwards to become the decidua reflexa, will contribute to the formation of the placenta. In this case by the distention of the ovum, and the yielding of the decidua reflexa, the placenta will come at last to be inserted over the mouth, or over some inferior part of the uterus."

Agreeably to this doctrine, the placenta must be found in one of three places, namely, over the opening of either fallopian tube, or at the inferior part of the uterus. Does this accord with experience? the gentleman himself, I am sure, upon reflection, will say no. There is certainly no part of the internal surface of the uterus to which the placenta may not be attached; and I believe it will accord with the observation of every accoucheur, when I say, I do not think any one part of the superior three-fourths of that viscus, seems more especially selected for its implantment. Baudelocque\* in combatting the opinions of Mons. Levret on the causes of the obliquity of the uterus, furnishes us with many examples of uncertainty, to what part of this organ the placenta may adhere. And Mons. Levret is of opinion, that the obliquity of the uterus happens whenever the placenta is not attached to the centre of the fundus; now if this be true, it will demonstrate how rarely it is grafted there, since almost every woman is subject to this complaint. But we will not insist on this negative proof of the variety of places to which this mass may be attached, since the observation of every day affords abundant evidence of it.

But let us examine for a moment the manner in which we suppose the ovum to pass into the uterus. The ovum, we suppose, when detached from the ovarium, must be

\* Midwifery, Vol. I. Art. Obliquity of the Uterus.

completely globular, and that its membranes possess a considerable degree of firmness, that it may not be burst by the action of the fallopian tube ; now if it be completely spherical it must revolve frequently in its course down the tube, as we presume it is furthered along by that canal repeatedly contracting from behind ; if this be true, to what part of the genital system shall we attach the power of measuring exactly the length of the tube ? to determine the precise number of revolutions it is to make, that “ the part of the membranes to which the embryo is attached may enter the uterus last ;” to nicely adjust its situation before it enters the tube, that the case may not be reversed, and the embryo be made to “ enter first.” I say to what part shall we give this power ? for without all these precautions we shall see at once that the ovum must enter the uterus promiscuously ; this in fact we suppose, and hence we find the placenta may be attached indifferently to any part of the internal surface of this organ.

From a note in p. 153, we are led to suppose that Mr. B. does not entertain the same opinion of the shape and firmness of the ovum as is contended for here, since he says, “ The entrance of the ovum may be compared to the delivery of the child, at full time. In both, the membranes protrude first ; at least, they always protrude first in labour, owing to the bulk of the child ; and most commonly they protrude first from the tube, although, from the smallness of the embryo, this does not invariably happen.” I shall not remark on the glaring inconsistency of the comparison, but merely state, what strikes me as the opinion of M. B. of the form of the ovum. From what he advances in the note just quoted, we are led to suppose, he thinks the passage of the ovum a species of labour, and that the membranes of it are lax or not completely distended, since they are pushed before the embryo, like the membranes at full time before the advancing child. Now, if this be admitted, what is to preserve the tender embryo from the destructive embrace of the contracting fallopian tube ?

I conceive that Mr. B's ideas on this subject cannot be

admitted, as the most trifling compression would be sufficient to destroy the embryo; and agreeably to his own statement the ovum must necessarily be subjected to considerable restraint, since the decidua lines the tubes for an inch, which must serve very much to diminish their capacity; and also subject the ovum to sufficient pressure to lengthen and push out the membranes before the embryo.

Besides, we have the authority of most of the experimentalists on this subject, to say, that the ova are completely round, if any dependence is to be put upon their figures of them; those of De Graaf and Cruikshank are decidedly so, and we trust that reason confirms the opinion. It will now irresistibly follow, that Mr. B's statement of the manner in which the ovum enters the uterus is chimerical; and that the ovum must frequently apply different portions of its surface to that of the tube during its passage through it, consequently, no certainty can obtain with respect to the part it may present to the opening at the uterus.

## A CASE

OF THE ALTERATION OF THE COLOUR OF THE HAIR  
DURING PARTURITION.

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THE following case is communicated more from the peculiar circumstances which attended it, than from its possessing any particular practical importance.

On the 21st February, 1804, I was requested to visit the wife of Reuben Elliot, whose labour was attended with convulsions. I found her under the care of a midwife, who gave me the following account of her:—"She had been called to her the evening before, but found very little appearance of labour except pain; the os tinæ was not dilated; there was no secretion of mucus; the pains were irregular in recurrence and force. At five in the morning, (eight hours after her first seeing her) she was seized with convulsions; soon after there was a discharge of the waters; pains continued as before, about an hour after the first fit she was again seized, and the convulsions were, after this, repeated with considerable frequency and great violence." At nine o'clock, P. M. I was sent for, that is, four hours after the commencement of the fits. I found the uterus but very little dilated, so little so, as not to be able to determine the precise situation of the head.

During my stay, the convulsions were very frequent and severe; I conceived that nothing but extensive and repeated bleedings would relieve her; her pulse was full, tense, and strong, in the intervals of the fits; but was extremely frequent, and almost imperceptible, when they were about to cease; the skin was hot and dry, except at the close of a convulsion, at which time it became cold and



clammy; thirst great; when interrogated appeared much confused; complained at times when roused of great pain in the head; the breathing laborious, and sometimes, especially immediately after the fit, stertorous; the face very much swelled, and perfectly livid during the paroxysms; as the lividity wore off, the pulse became more expanded, less frequent, and hard, and this took place in proportion to the restoration of respiration.

Many times my patient was threatened with complete suffocation, either from spasm, a great quantity of bloody mucus that seemed to fill the trachea, or both.

As nothing could immediately be done as I conceived, but bleeding her extensively, I sent Mr. Turpin and Mr. Stark, two gentlemen who were then attending lectures in the city, to stay with her, and bleed as much, and as often as should be necessary, either to suspend the fits, or to dilate the os tinæ sufficiently to allow of artificial delivery. This required a considerable quantity of blood; about fifty ounces were taken at four bleedings from ten o'clock in the morning, until five in the afternoon. On my calling at this time, I found the uterus sufficiently dilated to admit of delivery, and proposed employing the forceps, in preference to turning, as the waters had been long drained off, although the head had not yet passed the superior strait.

The pains and convulsions were now less violent, and less frequent; and the woman perfectly sensible in the intervals. I sent her husband for my forceps, while I visited a patient at some distance. On my return I found the patient in the greatest affright, as the midwife had incautiously alarmed her about the use of the instruments, and she was soon after seized with a very violent fit, which I have no doubt was hastened, and protracted by my presence.

On my going to the bedside I observed a whiteness on the fore part of her head which I had not observed before, and called for the candle to view it more particularly, by which, I discovered that the hair anterior to the coronal

suture was changed completely white, excepting where it was here and there interspersed with locks less completely blanched of its natural hue. I called the attention of the two gentlemen above mentioned, as well as that of the women present to it, and they all agreed it must have changed in a very short time, and since my last visit.\* I immediately proceeded to deliver her with the forceps, which I effected in a short time. She had but two fits after her delivery.

On my visiting her the next day, I found the hair much less white than the preceding evening, and in about four or five days it became nearly natural. I was informed a few days since by the midwife who attended her at that time, and has since delivered her, that the hair that had undergone the change, remains lighter than the rest of her hair.

To what circumstance shall we attribute this change of the hair? Did it arise from fright or anxiety? or did the convulsions themselves exert a particular influence on this portion of the scalp?

I am inclined to believe it must have arisen from some peculiar operation of the mind, as we have upon record similar instances, wherein it pretty evidently appears that terror has operated this effect; I shall therefore take the liberty of relating two. Schenkij† relates the story of Don Diego Osorius, a noble Spaniard, being in love with a young lady of the court, had prevailed on her to a private assignation in the garden of the king; they had been there but a short time before they were betrayed by the barking of a dog; Diego was seized by the guards and thrown into prison: as it was a capital offence, he was condemned to die—he was so terrified at hearing his sentence, that in the course of the night he became grey as if far stricken in years, which so moved the king's compassion as to pardon him.

Boyle‡ tells us of a captain in the Irish army, who was about to deliver himself up to Lord Broghil the com-

\* I was absent about an hour, or perhaps not more than three quarters.

† As quoted by Turner, lib. 1. p. 2.

‡ Exper. Philos. Vol. 1. p. 90.

mander of the English forces, agreeably to a proclaimed pardon, to those who would return to their allegiance, being intercepted by the party of the English ; the governor being absent, the poor fellow became so alarmed lest he might be put to death before his return, that his hair became white in parts, while others retained their original red colour. These cases would seem to point out a particular influence the mind has upon the hairy scalp, but how this influence is exerted we cannot pretend to say.

There is one circumstance in this case, and indeed in all others of the kind as far as I have observed, that is worthy of notice—which is, the almost imperceptible pulse near or at the cessation of the convulsions, and its gradual augmentation of volume, and diminution of frequency in proportion as respiration becomes better established. Does this not militate much against Dr. Beddoes' idea of the pulse becoming frequent in proportion to the oxygenation of the blood ?

ACCOUNT OF THE USE OF THE

VOLATILE TINCTURE OF GUAIAIACUM,

IN PAINFUL AND OBSTRUCTED MENSTRUATION.

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As few diseases to which the human female is subject are more obstinate than painful menstruation, and an obstruction of that discharge, I have been tempted to give a short account of the use of the volatile tincture of guaiacum in these diseases.

Painful mensuration is a disease but too frequent in our climate; whether this arises from any peculiarity of temperature, or from less regard being paid to this discharge, during its flow, by our incautious females, I will not pretend to determine; but such is its frequency and obstinacy, that it merits the particular attention of every practitioner, and more especially of those who have more immediately this class of sufferers under their care.

I shall not enter into a history of this disease, as it is sufficiently described by Cullen, and other systematic writers. I shall, however, advert to one symptom of this complaint, first noticed, I believe, by Dr. Denman,\* as it is an important one in its history; I mean the discharge of a kind of membrane during the period of the secretion of the menstrual discharge. The casting off of this coat is not, however, invariable with women who menstruate with difficulty; but where it has obtained in married women, my experience goes to confirm the remark of Dr. Denman, that

\* Morgagni also mentioned this, and long before Dr. Denman, but at the time of writing this paper it had escaped me.



they have been invariable barren.\* This symptom, if I may so term it, is, I think, more frequent in the country than in cities, and in that particular part of the country in which I first settled,† the women were particularly subject to it; consequently many in that neighbourhood were barren. I had tried a great variety of remedies for this complaint, without ever in a single instance removing it, until I employed the tinc. gum. guaiac. vol.—The only thing that could be done, was to abate the violence of pain by bleeding, warm bath, laudanum or camphor, &c.

After having been a long time baffled in this disease, I determined on the use of the medicine just mentioned. I was led to its employment, from supposing the disease to be a rheumatic affection; my reasoning on the subject was short, and perhaps fallacious, but the consequences of it were certainly favourable—I supposed the uterus a muscular part, performing certain specific actions important in the animal economy, and that during this period, it was but too frequently subjected to considerable variety of temperatures, the immediate effect of which for the most part was, either to suppress entirely, or very much diminish, the secretion of the menstrual blood, and that in consequence of this, the same condition was produced in the uterus, as in any other muscular part improperly exposed; that is, it became rheumatic. I will not pretend to point out in what this consists, as I believe we have not yet a sufficient number of observations on parts thus circumstanced, to enable us to offer any thing more than conjecture; I shall not, therefore, hazard one, more especially as the term rheumatic, if it does not ascertain the precise state of the parts, yet sufficiently instructs the practitioner as to the disease itself, and the nature of the remedy to be employed. With a view to obviate or overcome this condition of the uterus, I gave the guaiacum, and it has in almost every instance, answered my most sanguine expectation, nay, at first, went even beyond it: for I did not cal-

\* In Morgagni's case this did not obtain.

† Abingdon, about 10 miles north of Philadelphia.

culate upon any thing more than relieving the great pain of menstruation ; but I found, that this was no sooner removed in married women who had hitherto been barren, than they conceived ; of this I could give many remarkable instances. In most of the cases of barren women, who were afflicted with painful menstruation, there was the casting off of more or less of the membranous substance just spoken of ; when this would come away, pain would soon after abate, and presently cease altogether ; but for the most part, there was no abatement in the distressing symptoms until this happened. The presence of this substance acts on the uterus like every other that becomes an extraneous one, by exciting it to violent and painful contractions until the offending cause is removed. The pains attending this complaint are periodical like those of labour, but more permanent in their continuance, and more violent in their degree ; in some cases I have witnessed, the exhibition of four or five hundred drops of tinct. opii before the pain has been even mitigated : I have known it sometimes attended with convulsions, and at others, with most violent vomitings.

I have just observed, that every case of dysmenorrhœa is not attended with the discharge of this membrane like substance ; this ought to be admitted with some caution ; and it would be more proper perhaps to say, it is not observed in all cases ; for I am inclined to believe the same disposition exists in the uterus, only in a lesser degree ; but to a sufficient extent, in many instances as to entirely prevent conception ; but the same remedy is equally proper in both cases.

Are there any cases in which this remedy is not eligible ? hitherto I have met with no such case ; they may exist. This medicine should not be given with an active pulse ; if this exist, it must be reduced by bleeding, purging and low diet.

I begin the use of the tincture in the following manner : a tea-spoonful three times a-day, in a glass of Madeira, Sherry, or Lisbon wine, cider or milk ; I generally direct

it to be taken before each meal, and continue it in this way, unless it happen to offend the stomach when taken before breakfast; in this case, I order it an hour after. I commence its use at any period of the interval of menstruation, but discontinue it during the discharge: but so soon as this is over the tincture is again given. It sometimes requires a perseverance of three months to effect a cure, and during this time, the quantity is to be gradually augmented to three tea-spoonfuls at a dose. Should it prove purgative, a little laudanum must be added to restrain its effects on the bowels; should it not be sufficiently aperient, a little resin of jalap or powdered rhubarb may be used with it; or have recourse to the occasional use of the oleum ricini.

There are some women labouring under this complaint, who, during the menstrual period, will require blood-letting; it must therefore be remembered, that the pulse be kept sufficiently down during the exhibition of the tincture. To those who are plethoric, an abstemious diet is necessary, and the occasional use of the warm bath has been found serviceable.

Flannel next the skin, and a strict attention to keeping the legs and feet warm, are particularly recommended.

During the flow, camphor given in the following manner, rarely fails to give immediate relief:

R Gum. camph. ʒ j.  
 — arab, ʒ j.  
 Sacch. alb. q. s.  
 Aqua cinam. simp. ʒ j. m.

one half to be given as soon as pain comes on, and if not relieved in two hours, the other half is to be given; which for the most part is sufficient. I have in some instances been obliged to give laudanum after the camphor, but not often. Should vomiting attend, camphor and laudanum should be given by enemata, as follows:

R Gum. camph. ʒ ss.  
 Sp. vin. rect. q. s. f. pulv. add  
 Tinct. thebii. ʒ j.

This to be mixed with a gill of thin starch, and given as a clyster; this may be repeated *pro re nata*.

I will not pretend to account for the operation of the camphor in this disease, but its effects are very remarkable, in not only relieving pain, but diminishing, and in some cases entirely preventing the discharge of the membrane. I was taught the use of this remedy by an old woman who had laboured under this complaint, and who in a fit of desperation, in one of its paroxysms, drank a wine-glass-full of camphorated spirit, which to her great surprise and joy instantly relieved her; since, it has been recommended in the above and more elegant form, by a gentleman in the *Medical and Physical journal*.

In two cases where I failed with the tincture, hemlock was useful; and in one other, the tincture of cantharides gave effectual relief.

#### OF ITS USE IN OBSTRUCTED CATAMENIA.

I shall only notice in my account of the use of the tincture of guaiacum in obstructed menses, those cases which I think may strictly be considered as chronic, and idiopathic. It has been usual, more especially of late, to regard obstructions of this kind, as merely symptomatic; an error, I conceive, of some magnitude. When we reflect on the important and independent functions the uterus performs, we shall not hesitate in allowing it diseases peculiar to itself, among which we must regard the amenorrhœa. In this kind only would I recommend the guaiacum as a remedy. In diseases of the system at large, or of any particular viscus, with which the uterus may powerfully sympathise, this medicine is not to be depended on, or at least not until the original disease be removed; thus we find in phthisis pulmonalis, schirrous liver, &c. that the uterus ceases many times to secrete the menstrual blood; in these instances it would be more than in vain to employ the tincture of guaiacum. But where the interruption to the secretion has had no other remote cause than exposure to



cold, just before or after the time for its discharge, or fevers without visceral obstructions, this remedy, I can with safety declare, from an experience of sixteen years,\* that it never has in a single instance failed me : I look upon it more certain than bark in an intermittent. After the menses have failed two or three periods, they very rarely return spontaneously ; if neglected long after this period, they lay the foundation of various unpleasant symptoms, and sometimes of serious ill health. If then a bleeding, a brisk purge, warm teas and warm bath, do not restore them at the first or second accustomed period, we should immediately begin the use of the guaiacum. In some cases it is necessary to prepare the system as it were for its use ; that is, with women who are robust and plethoric : for this purpose blood-letting, purging, and a vegetable diet should be premised a few days, or until the system will bear the stimulus of the tincture. When the system is thus fitted, it is to be given as above directed for painful menstruation ; and with the same precautions and exceptions. It sometimes relieves very quickly, at others it will require a perseverance of five or six weeks, but it rarely employs as much time as the disease just spoken of.

I have known this remedy in two instances restore this discharge, where it had ceased three years, and many where it had failed more than one.

As the tincture I prepare is something different from the tincture of the shops, I have subjoined my formula.

R	Pulv. gum, guaiac.	℥ viij.
	Carbon. sod. vel potas.	℥ iij.
	Pulv. piment.	℥ ij.
	Alcohol. dilut.	℔ ij.

Dig.

The volatile spirit of sal ammoniac to be added, pro re nata, in the proportion of a drachm to every four ounces of the tincture ; or less or more agreeably to the state of the system.

\* I may now say of two and thirty years.

## REMARKS ON

### DR. PEACHEY HARRISON'S REPLY.

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DR. H. in his reply observes, p. 98, that "it cannot be regarded as presumption in me to believe that the Doctor has embraced an hypothesis," &c. ; on the words "embraced an hypothesis," he remarks in a note "I say embraced, because I am able to shew he is not the inventor of it." The inventor of what ! I would beg leave to ask ? Have I claimed originality for the doctrine of the possibility of superfœtation ? Certainly I have not, as Dr. H. may very readily satisfy himself by turning to the essay ; he will find I say it "is not a new idea," and that the express object of the essay "is to revive it." Nor do I any where claim the doctrine of absorption ; as every one knows, who is in the least conversant with the subject of impregnation, that this honour is due to Harvey. It is true, I have offered a modification of this opinion, by supposing this operation (absorption) may be affected by a set of vessels whose sole duty is, to convey the ejected semen to the ovaria ; and this, of however little consequence it may be, or however absurd it may appear, I believe to have originated with myself ; and I also believe, that no one before me, has attempted to deduce the possibility of superfœtation, from the premises I have adopted. From this, it would appear, that I claim but the humble merit of arranging in a new order, facts and opinions, known to many before me ; and this I shall continue to believe, until I am shown that I am not entitled to priority in this arrangement.

Dr. H. is certainly wrong in supposing, my object was to call in question the classical purity of the expression, of "extorting from nature her oracles" in my remarks on it; it was done with a view to set him against himself, when in one place he flatters himself he has done this (extorted nature's oracles) not altogether without success; and in another, launches into an ocean of conjecture, which eventually I think overwhelms him; the oracles of nature should be pure and unsullied by conjecture. Am I not then correct, when he substitutes hypothesis for the "oracles of nature," in saying, they, in my opinion, are without foundation or plausibility?\*

In the same paragraph, p. 100, Dr. H. observes "may I be permitted to inquire, upon what foundation his (Dr. D's) hypothesis rests?—upon facts? I declare, that after reading his essay upon superfœtation repeatedly, and with the utmost attention of which I am capable, I am unable to find any from which his hypothesis could directly be inferred; and he confessedly admits, that he takes those facts for granted upon which he builds his hypothesis. Oh! but he first proves, that superfœtation cannot take place according to another hypothesis, and therefore it must take place according to the hypothesis he has suggested! What kind of philosophy is this I beseech the Doctor? Has he learned this mode of reasoning from Lord Bacon or from Sir Isaac Newton?"—There is in this quotation an ambiguity and want of consistency that is well worthy of attention. I cannot understand the Doctor, when he says I have confessedly admitted, I have taken my facts for granted. Have I not given authorities for the major part of them? They are therefore not assumed; and if I understand the import of the word assumed in this instance, it means, I have de-

\* Dr. H. need not have had recourse to the Mantuan bard for the employment of the term, as Spallanzani, much nearer our time, makes use of the same words and for the same object. He says, p. 244, disser. II. Par. clxxi. "In this branch of physics, we must not generalise our ideas, but are under the necessity of consulting the oracles of nature, and receiving her answers with respect and attention."

clared, I have taken for granted, circumstances that may not be true; now no man in his senses, be his sincerity what it may, would declare he did not believe in his data, when he was about to draw an inference important to his doctrine. But I will go farther, and here declare, I do believe implicitly in the facts quoted, and consequently do not beg them. Is it not "illogical" to conclude that facts must be "assumed," because Dr. H. cannot find any, from which my hypothesis can be directly inferred? This does no more than call in question the force of the facts, but surely does not destroy their truth. Besides, Dr. H. admits a little farther, that I prove "superfætation cannot take place according to another hypothesis," yet affects to call in question the facts on which this proof rests; how does this harmonise?

I shall now, for a moment, advert to a part of the above quotation, on another score. Dr. H. says, in a manner sufficiently unqualified, that I have concluded, because superfætation cannot take place according to another hypothesis, that it must take place according to the one I have suggested. I do declare I have said nothing like it, nor is it in his power, with all his logical talents to aid him, to shew, that such a conclusion will follow my premises. Were it so, the Doctor might well exclaim, "What kind of philosophy is this." I trust I have, to every unprejudiced mind, shewn how it cannot take place; and then have humbly attempted to show how it were in my opinion possible; if this be a crime I plead guilty; but I conceive every man has a right to call in question, doctrines however long received, or however sanctioned; but in doing this, it is also incumbent, to offer one in the place of that he has endeavoured to repudiate. This I have done, but not in the dogmatical style Dr. H. imputes to me. Dr. H., p. 101, thinks he has ascertained "from experiments, repeated frequently, patiently, and perseveringly, and instituted with a view to ascertain the point, that venereal sensibility has its principal seat in the os tincæ;" but acknowledges he is unable to support the fact by the direct



testimony of any authorities to which he *has access*. Does he know of any authority either ancient or modern for the assertion? I must confess I do not. He then asks, was it in the power of "Harvey to substantiate the fact of the circulation of the blood, by the testimony of other writers?" I say yes, and upon those who had gone before him chiefly; it was upon their discoveries and facts, that he first deduced, and then proved, the circulation. Had no one preceded Harvey, had he not had multiplied experiments and facts to have rested on, he would not have immortalised himself by his discovery. Did he discover that the heart had two auricles and two ventricles, which contracted and forced the blood from it? Did he discover there were two sets of vessels belonging to the sanguineous circulation? The same question may be asked of almost all the anatomical facts connected with this discovery. Harvey has then the merit of employing the knowledge of other anatomists, so as to produce the most important discovery in the history of medicine. And had not Jenner had public and living records to prove that the natural vaccine disease when received into the human constitution prevented the small-pox; we should not now have been benefited by his discovery that this obtains equally, when artificially introduced.

Dr. H., p. 102, appears to have some difficulty in understanding what he terms a "perplexed chain of propositions;" this I conceive must be his own fault, since I will venture to say, they stand in no such predicament with any one else; they appear to me plain and conclusive, and this I assign as the reason why Dr. H. found them perplexed. But I would beg leave to ask by what rule of grammar or "logic" will Dr. H. make "very many" mean most? I have not ventured to rely on my own judgment with respect to what I conceived to be the obvious meaning, but have taken some pains to inquire of several of my literary friends, on whose opinions, as far as regarded this question especially, I could safely depend, without being able to find any authority for Dr. H's con-

struction. But, lest this should again be cavilled at, I now explicitly declare, that my meaning was, that a considerable number was intended to be understood.

In p. 103, Dr H. leaves to logicians to decide, whether the inference drawn from "the perplexed chain of propositions" be properly made from the premises; "and whether the reasoning, if it conclude against any thing, does not more logically conclude against the necessity of pleasure in the prolific intercourse, than against the seat of the venereal sensibility being in the os tinæ. If there are indeed women, who, when they are in a situation to be impregnated, feel no pleasure in the sexual intercourse, and are yet prolific, I grant this proves that pleasure is not an indispensable requisite in fecundation."

Let us now examine Dr. H's theory of impregnation, and see how it will square with the above concession. Dr. H. p. 423, Vol. II. No. viii. gives "the following combinations of causes and effects, to convey *a clear conception* of the manner," in which he supposes impregnation to be effected.

"Irritation applied by the glans penis to the os tinæ, well prepared to receive this irritation by its projection into the vagina, and by its extreme sensibility, is the immediate cause of the venereal orgasm, or that unknown desire which the female sometimes feels, in coition, for the seminal stimulus; and secondly that *an absorption* of the fecundating fluid, when applied to the os tinæ during the orgasm, is the proximate effect thereof; and in the third place, that the transmission of the semen to the ovaria, *by the proper action of the uterine system*, is as naturally the consequence of absorption, as deglutition is of agreeable aliment taken into the pharynx. And lastly, that impregnation is the final cause of the sexual intercourse, and of the *pleasures* with which it is accompanied."

"It clearly follows, from what has been stated above, that impregnation *will never take place*, unless the venereal orgasm has been excited: and it is equally plain, that it will not happen, unless the semen is brought in contact with

the os tincæ during the venereal orgasm ; and it follows with *equal clearness*, that an apposition of the urethra to the os tincæ, at the same time, will be important, if not indispensable ; not indeed for the purpose of the injection of the semen into the uterus, but that the seminal stimulus be applied to the os tincæ, at the moment when it is prepared to absorb and carry it to the ovaria.

The opinion of Dr. H. is then "so far as I am able to understand, so perplexed a chain of propositions," first, that contact between the "velvet-like head of the penis," and the os tincæ. is essentially necessary, as appears from the expressions, "irritation applied by the glans penis to the os tincæ;" by its being "well prepared to receive it;" by its (the os tincæ) being "actually the seat of pleasure in females;" and from its "in the sexual intercourse" receiving "irritation from the soft and velvet-like head of the penis," &c. Now we have, in our former reply, said enough from the most respectable authorities, to convince any unprejudiced man, that many instances of impregnation have occurred, where it was physically impossible for this contact to take place. But lest it should be objected, that these obstructions may have arisen after impregnation had taken place, I will give an extract from Baudelocque's Midwifery, on the authority of Barbout, that I conceive perfectly in point. "Transverse partitions have often been found in the vagina ; and it has been known to open into the rectum, in women who wanted the external parts of generation, without this vicious confirmation having rendered them absolutely barren." Barbout, tom. I. p. 59.

This, with the examples we before cited, with the generality of men, would be sufficient to prove that instances of impregnation have occurred, where "irritation by the glans penis" could not be immediately communicated to the os tincæ ; but to do away all pretence to this essential, I will relate the following history, the truth of which may be relied on, as it was communicated to me by the lady herself, who certainly had no theory to support.

Mrs. G. aged 33, was seized on the 10th of April, 1807,

with a violent vomiting in consequence of a portion of intestine becoming strangulated in a hernia, which she had laboured under many years ; I was called to visit her, and in a few days she perfectly recovered ; during my attendance on her she mentioned, she had ever since her first labour been troubled with a falling down of the womb, for which she was obliged always to wear a pessary. I begged her to shew me one of her pessaries ; it was made of cork coated with wax ; it was about four inches long, two broad, and had a hole in the middle of the third of an inch in diameter. She had worn one ever since her first lying in, with the exception of a short period after each delivery, and after the sixth month of each pregnancy. She was the mother of six children. As this case was so directly in favour of the idea, that contact between the penis and the os tinæ was not necessary to impregnation, I was particular in my inquiries respecting the most essential circumstances. I asked, if she always wore the pessary as she had asserted, (with the exceptions just stated) how could she possibly have become pregnant ? she said she did not pretend to know how this happened, but such was the fact. Am I then to understand, without any equivocation, that you believe you ever have become pregnant while you wore the pessary ? She assured me, she was certain this had always taken place while it was within her, with the exception of the first child. I demanded, whether this was not an obstruction to conjugal union ; she said, nothing like as great as without it, for her womb was always so low, as to prevent entirely this intercourse.

Secondly. Dr. H. asserts, that from "its extreme sensibility, (the os tinæ) it is (in conjunction with irritation) the immediate cause of the venereal orgasm ;" now as we have, we think, sufficiently proved above, that impregnation has taken place where this irritation could not be applied to the extremely sensible os tinæ, no venereal orgasm could take place, as its proximate causes were wanting ; consequently the venereal orgasm, or that "unknown desire which the female sometimes feels in coition, for the



seminal stimulus," cannot be essential to impregnation, if, as Dr. H. supposes, the glans penis and os tincæ must be in contact for this effect to be produced.

I think, I do not force Dr. H's meaning, when I assert, he must consider pleasure or pain, essential to the venereal orgasm, and consequently to impregnation; since, agreeably to his own declaration, "impregnation will never take place unless the venereal orgasm has been excited." He admits the glans penis to be an irritating body; the os tincæ to be an extremely sensible body; now, what is the necessary consequence of the application of a body capable of irritating, to another body extremely sensible, must it not be either pleasure or pain? It is true Dr. H. has urged, p. 422, "that the venereal orgasm consists in a certain excitement of the uterine system, and is *accompanied* with exquisite sensations, similar to those which took place in the male;" but I cannot perceive the difference as respects the main or "final cause of sexual intercourse," whether the pleasure attending the act be the cause or effect, or neither, since it must accompany, and be present at the time the venereal orgasm exists; and I care not to which of these heads he refers it, since he agrees, that impregnation cannot take place "without venereal orgasm," and that the "venereal orgasm is accompanied with exquisite sensations." Pleasure, then, agreeably to Dr H's hypothesis is a necessary link in the chain of effects, arising from the irritating glans penis, when applied to the extremely sensible os tincæ.

Thirdly. That the semen must be absorbed during the venereal orgasm. It might not be amiss to ask Dr. H. how he supposes this absorption to be effected; for he certainly has not explained it by saying it does happen. His ambiguity and want of logical precision on this subject are very notable; for he conveys nothing certain to my mind when he says "that the transmission of the semen to the ovaria by *the proper action of the uterine system*, is as naturally the consequence of absorption, as deglutition is of agreeable aliment taken into the pharynx." For, before

we can admit this opinion, several things must be proved : first, that the venereal orgasm is produced in the manner urged by Dr. H.; secondly, that this is essential to impregnation ; thirdly, if it does take place as Dr. H. supposes, he must shew that this part when so circumstanced has the power of absorption ; and fourthly, that when it has absorbed, he must demonstrate it conveys the semen along in a manner analogous to swallowing, or that this *proper action* of the uterine system takes place.

Dr. H. p. 102, reduces my arguments or (agreeably to his own phraseology) my "perplexed chain of propositions" against the seat of venereal pleasure being in the os tincae to a dilemma, which he thus retorts, "now, unless that those women who enjoy pleasure in the venereal congress, and those who do not, have an organic difference in the part in which the venereal sensibility resides, we must conclude that *this part* is not the seat of venereal sensibility." This dilemma of the Doctor's might easily be again retorted, but as I have not pretended to locate venereal sensibility or enjoyment in any thing I have advanced, and as I do not believe in its instrumentality in conception, the onus probandi lies with him.

Dr. H. p. 104, endeavours to draw a parallel between the sense of feeling possessed by the hog in his snout, and the elephant in his proboscis, and the sensibility of the os tincae ; since he evidently confounds the sense of feeling, which these parts may have, even in an eminent degree, with sensibility strictly so called, I shall only observe, in general, the analogy between the sense of touch which distinguishes between bodies, and that state of a part termed sensibility, is too remote to serve as a standard of comparison, and too distinct ever to be confounded. Dr. H. a little farther on, says, "to these observations, (those respecting the hog's snout and the elephant's proboscis) it is hardly worth while to add, that its texture (the os tincae's) is certainly less firm than that of our teeth, which are as sensible to the variations of heat, perhaps, as any other part of the body."

It will be readily admitted, that the teeth are very sensible to cold, but not to heat,\* unless they are diseased. But I will not allow Dr. H. has any right to the comparison, since the bony substance of the teeth are mechanically acted upon by the contraction of the enamel which surrounds them. In consequence of the contraction which the enamel suffers from a reduction of temperature, the nerve of the tooth is impinged upon, and no one will deny but a living nerve possesses sensibility.†

I am sorry to be under the necessity of accusing Dr. H. of misquoting me in the next paragraph; I am rather disposed to believe, from his general appearance of candor, that it must have proceeded rather from negligence than design. He makes me say "as far as can be determined by the *sense of touch*," whereas, I say by the touch. It is well known to accoucheurs that by *the touch* is meant the use or employment of the finger, to ascertain the state of the uterus or os tincæ or both; therefore, I conceive there is no *Irishism* in saying, "as far as can be determined by the touch," or in other words, by the application of the finger, for which the *touch* is the technical term.

\* Heat and cold we are aware are only relative terms; but I have chosen to employ them here as separate properties for the sake of perspicuity; and when I say the teeth are susceptible of impression from cold and not from heat, I wish only to be understood as referring to the sensation they produce, without any reference to the absolute quantity of caloric a body may contain. Thus, then, a considerable degree of heat above that of the mouth, will not affect sound teeth disagreeably, whereas, when the temperature of the medium, offered to the teeth is considerably below the temperature of the teeth themselves, they will be unpleasantly affected; the reason I have elsewhere explained.

† We might notice with great propriety Dr. H's evident oversight of the anatomical arrangement of the ear and nose, when he demands "whether the texture of the os tincæ be firmer than that of the seat of the sense of hearing, or of that of smell? Does Dr. H. mean by the seat of the sense of hearing, the petrous portion of the temporal bone, or the portio mollis of the auditory nerve? Does he mean by the seat of the sense of smell, the nasal and ethmoid bones, or the Schneiderian membrane? It is certainly difficult to decide this point; since if he mean the portio mollis, it will bear no comparison with the os tincæ; and if he mean the bony canal in which it is encased, he must shew this part is the seat of hearing, and not the portio mollis. If he mean both the portio mollis, and Schneiderian membrane, it would be easy to prove, they are much less firm in their texture than the os tincæ.

Dr. H. lays it down, p. 105, as "an incontrovertible truth, that whatever part of our bodies is capable of being pained, may also be the seat of pleasure." Is not bone, ligament, tendon, cellular membrane, &c. capable of being pained, yet have they ever been known to produce or yield pleasure? Dr. H. farther says, he "will allow that the os tincæ may be freely felt by the finger, without exciting much either of pleasure or pain." What has become now of its "extreme sensibility?" Could any other part of the body to which the attribute of sensibility is allowed, be felt freely by the finger, without exciting much of either "pleasure or pain?" Dr. H. endeavours indeed to reconcile this seemingly contradictory concession, by observing, "But does it follow consequently, that the friction of the penis in coition does not excite pleasurable, and perhaps, in some, for aught I know, painful sensations? I need not undertake to prove, that there is such a thing as specific sensibility, and that specific sensibilities are only to be aroused into activity by their appropriate objects."

I will not pretend to deny the existence of specific sensibilities, but must say Dr. H. has not proved there exists one in the os tincæ: I will even go farther; I do not believe there exists any there. My reasons for thinking so are, first, pleasurable sensations appear to be excited where the "appropriate object" has not been applied; and secondly, when applied, under the most favourable condition, as far as can be determined by existing circumstances, the specific effect has not resulted.

To prove my first objection, we need but recollect the disgraceful means sometimes employed for this purpose; with a view to a more full illustration of what is here alluded to, I will transcribe a few passages from Champon, a modern French writer on the diseases of girls. He asks, p. 80. vol. II. "Est-ce dans les vices de l'éducation, dans la séduction des exemples dangereux, et dans la force du tempérament ou l'effect des passions, qu'il faut chercher



la cause de la masturbation ? Ne seroit-ce pas aussi quelquefois au concours de certaines circonstances qu'on pourroit attribuer l'origine du penchant qui porte un grand nombre de femmes à jouir d'elles-mêmes ?”

After entering into an inquiry on the object of these questions, he adds, “ J’ai dit que les exemples dangereux étoient une des causes les plus ordinaires de la masturbation ; c’est presque toujours dans les lieux où les filles sont rassemblées en grand nombre, que cette funeste habitude se contracte.

“ Quand je traiterai de la fureur utérine, je dirai quelles sont les revolutions qui se passent dans une fille d’une constitution vigoureuse, et d’un tempérament ardent ; on saura mieux pourquoi les sensations qu’elle éprouve dans les parties de la generation, la forcent quelquefois à jouir d’elle-mêmes ; on concevra d’avantage comment une inquiétude douloureuse qui fatigue ces organes, y fait porter la main, sans avoir même l’idée du libertinage. L’impression qui naît de ce contact devient un sentiment de plaisir, et la jouissance se consomme avant qu’on ait eu le temps de la réflexion. On juge bien que le souvenir d’un moment de delices, que l’excès de santé rappelle souvent à l’esprit, ne peut être oublié ; les jouissances se multiplient, l’habitude se contracte, et les oreilles se ferment aux conseils de la sagesse.”

The extract just given will prove, that there is enjoyment without the “ appropriate object,” and consequently, if pleasure or gratification be produced, by the means mentioned, it is not essential that the parts Dr. H. supposes indispensable for their production, should come in contact.

“ To prove my second objection, I must again urge, that there are women who feel no pleasure from coition ; yet with whom it is presumable, that the “ velvet-like head of the penis” and the “ extremely sensible os tincæ” might as readily and as reasonably come in contact, as where pleasure is the result of sexual union. That I may not appear to stand alone in this opinion, I will cite a passage from

Dionis,\* (Eng. trans. p. 106) who is a warm stickler for sexual pleasure in the immediate business of conception. "And I have met with some who declared that they had no notion of that pleasure, for which others daily hazarded so much; and assured me also, that they had been got with child without being sensible of pleasure in the least."

Dr. H. p. 106, says, "I have always understood it to be a general fact, that venereal sensibility is diminished during pregnancy, and that women are less disposed to admit the embraces of the male, during this period, than at other times." Were this a fact in its fullest extent, it would prove nothing for Dr. H's theory, since, agreeably to his

\* This old writer appears to have entertained very nearly the same ideas of conception that Dr. H. does; to prove this, we will run two or three parallels of the principle opinions.

## DIONIS.

By introduction is understood, the entering of the yard so erected into the mouth of the womb,† that is eager to receive it. p. 74.

These parts feel at that time a mutual tickling pleasure, produced by rubbing one on another. p. 74.

Then emission is ardently wished for by both parties, as the height of pleasure and full enjoyment. p. 74.

The seed emitted directly into the mouth of the womb, is greedily received, and by the contractions of the uterus is pushed through the tubæ fallopianæ, to the ovarium. p. 75.

For I find that their action (the ligaments) is to draw the womb downwards, and by their elasticity, to bring it near to the yard in the act of generation, that by its internal orifice it may more conveniently receive the seed emitted. p. 34.

I have made it appear that the use of the ligamenta rotunda, is to bring the bottom of the womb forward towards the yard for the reception of the seed; the seed being received, the womb contracts, the seed is compressed and forced into the tubæ fallopianæ. p. 76.

† By mouth of the womb he means vagina, as he says, these two parts are as well fitted to each other as a sheath is to a knife, hence the mouth of the womb is called vagina.

## HARRISON.

That the venereal sensibility resides in the os tincæ, &c. I infer from the structure of the parts, and especially from the projection of the collum uteri into the vagina, where it will receive in the sexual intercourse, irritation from the soft and velvet-like head of the penis, well calculated to produce the venereal orgasm; which consists in a certain excitement of the uterine system, accompanied with exquisite sensations, similar to those that take place in the male, with an unknown desire to receive the seminal stimulus. p. 422.

That an apposition may take place, appears to me entirely probable from the phenomena, &c., the cervix uteri becomes turgid, the ligamenta rotunda contract, the uterus is depressed in the pelvis, the os tincæ is brought nearer to the orificium externum, and assumes a direction favourable to an apposition. 424.

That the transmission of the semen to the ovaria, by the proper action of the uterine system, is as naturally the consequence of absorption, &c. p. 423.

own position, the *os tincæ* is always within the reach of the penis during pregnancy. But the fact certainly stands otherwise, and I have no hesitation in saying, I believe it almost universally obtains, where affection makes the ground work of love; or where this act is not yielded to, rather as a duty, than as a source of gratification.

This fact ought to be perfectly reconcileable to Dr. H's theory if it were a just one, since, if the *os tincæ* be the actual seat of venereal enjoyment; since we know that sensibility is increased (*cæteris paribus*) by an increased flow of blood to the part; and since, by his own quotation from Dr. Monro, Dr. H. believes this determination to take place during the venereal congress; and since, this determination can only manifest itself by a distension of vessels; we think we have a right to conclude, that whatever will increase the determination of blood, will increase the sensibility of the part to which it is determined; and as we could very easily shew that this takes place with respect to the neck of the uterus, in proportion as gestation advances; and as Dr. H. insists that the *os tincæ* is always within the reach of the penis, we conceive, it must necessarily follow, that were the *os tincæ* the seat of venereal pleasure, it ought, agreeably to the Doctor's hypothesis, to augment in proportion to the advancement of gestation. Indeed, I am surprised Dr. H. did not take advantage of the fact we have insisted on, namely, that "women in the latter months of pregnancy, who do feel pleasure from sexual intercourse, have equal enjoyment when the *os tincæ* is entirely obliterated, or out of the reach of the penis."

Dr. H. p. 106, acknowledges, that "if the facts upon which this position rests," could be established, it would be entitled to great weight. Let us endeavour to satisfy the Doctor on this subject. I will try to shew from various authorities the changes which the *os tincæ* undergoes from pregnancy; and also attempt to prove, that it gets beyond the reach of the penis in the latter term of gestation.

"In the sixth month the neck begins to enlarge at its basis, and seems a little softer than before." Baudelocque, p. 245. Sect. 406.

"In the seventh, the neck grows still shorter, and becomes less accessible to the touch, *because it recedes from the vulva in proportion as it is developed.*" Ibid. Sect. 407.

"At the end of the eighth month its neck is almost always effaced; and its orifice so far off, that the finger can scarcely reach it, and to do it, we are obliged often to carry it as high as the sacro iliac symphysis, *right or left.*" Ibid. Sect. 409.

"In order to reach so far, we must proceed in the following manner, the woman must be *standing*," &c. &c.; he then gives a particular direction how the os tinæ may be touched, but as it not immediately connected with our subject, I have omitted transcribing it. Ibid. Sect. 410.

"Some accoucheurs recommend placing the woman on the bed, to touch her in the latter periods; in order, as they say, to bring the neck of the uterus to the centre of the pelvis, by diminishing the obliquity of its fundus. But we must not expect any assistance from this precaution; and it is *almost always impossible to reach the orifice* in that manner, and it is much better to touch her standing." Sect. 411. From this it would appear, that the weight of the uterus was necessary to bring it within reach of the finger; this will be a sufficient comment.

"In the last period of pregnancy, the neck of the uterus is *completely developed.*" Ibid. Sect. 412.

"If the cavity of the uterus becomes still larger after this time (the seventh month,) it is all at the expense of these fibres, (fibres of the neck) now become weaker. At first they distend and lengthen: then they seem to range themselves by the side of each other; which renders the uterus *so thin in this part*, that the edges of its orifice are often no thicker than two or three folds of common paper." Ibid. p. 133. Sect. 202.

"In the ninth month the neck of the uterus is altogether distended." Smellie, p. 96.



"The fundus of the uterus is the part first distended, and afterwards the inferior parts in regular order ; at length the cervix is obliterated, except the mere circle of the os uteri." Denman, p. 247.

"In the last weeks, when the cervix uteri is completely distended, the uterine orifice begins to form an elliptical tube, instead of a fissure ; and sometimes, especially when the parietes of the abdomen are relaxed by repeated pregnancy, disappears entirely, and is without the reach of the finger in touching." Hamilton, p. 89.

"And it becomes thinner and thinner, (the uterus) and in the last days is like the other parts of the womb, and is not distinguishable, but by its circumference." Dionis, p. 113.—"Tellement que quand la femme approche de son tems, il est (the mouth of the uterus) tenet applani, et presque confus avec la globe de la matrice." Mauriceau, p. 97.

From the quotations just made, I trust Dr. H. will be convinced, that the situation, and condition I had given the os tinæ was not ideal, and made to square with a preconceived opinion.

Dr. H. p 107, asks, "but is it a fact that the os tinæ even in the latter months of pregnancy, gets entirely out of the reach of the penis?" I have sufficiently answered this question I trust by the extracts just given. It must not however be concealed, that in some instances at the latter period of gestation, the uterus loaded with its contents is precipitated pretty low into the pelvis ; this however will make nothing in favour of the hypothesis we are examining, since when it does happen the woman suffers much from any attempt at connexion. Dr. H. in the same paragraph makes the following quotation from *Monro's anatomy*. "Moreover, (says the Doctor,) the cervix or neck of the womb itself, which has long remained unchanged, becomes much shorter during the last months of pregnancy, and at length forms a broad flat opening, which, towards the time of parturition, grows continually wider." "This" says Dr. H. "I take to be the true state of the

case ; but can it be inferred from this passage, that the os tinæ, even in the latter periods of pregnancy, is entirely out of the reach of the penis ?" certainly it cannot, Doctor.

I should be obliged to Dr. H. if he will point out any part of what I have written, in which I have declared, or intimated, I drew my conclusion that, the os tinæ was "entirely out of the reach of the penis" from the authority of Dr. Monro, or from no better or pointed authority than the quotation he has chosen to make. No ; I have drawn my opinion from accoucheurs of the first respectability, and my own observation. Dr. Monro was a good anatomist, but was no accoucheur, as the passage Dr. H. has given us clearly proves.

From the triumphant manner in which Dr. H. has ushered in the authority of Dr. Monro, he would seem to declare his certainty of victory from it, for he immediately adds, "now if the os tinæ is within reach of the penis through every stage of gestation, and I think it would not be easy to evince the contrary, the Doctor's argument will be found to have but little force."

From the above quotation, it would appear that Dr. H. has drawn, an inference favourable (as he supposes) to his opinion, from Dr. Monro's statement of the situation of the os tinæ. Let us examine for a moment his title to it ; his position, if I comprehend him (and I should be sorry to give a wrong interpretation to his meaning) will stand thus ; towards the latter periods of pregnancy the os tinæ "forms a broad flat opening," which gradually becomes wider ; now, as it grows broad and flat towards the later periods of gestation, it cannot be out of the reach of the penis. I will ask if this be logic ? So great a stickler for logical precision as Dr. H. should have been careful how he departed from it so widely.

Has not Dr. H. asserted rather much when he says, "this assertion (pregnant women feeling pleasure) is certainly contradicted, by the experience of *all* whose marriages have been fruitful." Has Dr. H. examined any kind of proportion of those "whose marriage has been fruitful ?" And

because "it is pointedly and unequivocally contradicted by analogy" in the Dr's. opinion, as "all brute females" have "an irreconcilable aversion during pregnancy, to the embraces of their males," does it follow it cannot obtain in the human female? Is she not entitled to her peculiarities as well as the brute? "In these," says the Dr. "impregnation seems completely to suspend, for a time, the venereal sensibility."

Thus, then, Dr. H's analogy will not bear him out in the most material circumstances, and were we to reduce his analogy to syllogism it would or ought to stand thus: in the brute, the venereal appetite recurs at stated periods, and if it becomes prolific at this time, its venereal appetite ceases; therefore the human female must obey the same law. "What kind of philosophy is this I beseech the Doctor? Has he learned this mode of reasoning from Lord Bacon or from Sir Isaac Newton? I imagine not." Now, the Dr. admits, in the brute the venereal appetite is suspended, or in other words, "the brute female has an irreconcilable aversion, during gestation, to the embraces of the male;" we will go farther and say they very rarely admit them. But does this happen with the human female? Does she not admit of the embraces of the male? She certainly has no "irreconcilable aversion" from this cause to conjugal enjoyment. And does she not when she admits of this, even agreeably to the sentiment of Dr. H. feel some pleasure? Does Dr. H. not expressly declare, p. 106, he "would not be understood, as denying that women may have considerable enjoyment from sexual intercourse; as it "is perfectly agreeable to his own observations?" What analogy then subsists between the brute and human female in this particular? Not the most remote.

With respect to what Dr. H. urges in p. 109, respecting the sensibility of the os tincæ, and his comparison of it with the extremities of the fingers and point of the tongue, I could say much, but as *every* thing we know on this subject may not be fit for a discussion like the present, which is to meet the public eye, I shall pass it over in silence,

pledging myself, that, should Dr. H. think the topic worthy a future consideration, I will freely enter into it by a private correspondence, wherein I can be more explicit, and perhaps more satisfactory.

I have urged that "the os tincæ having no fixed place in the pelvis, is by no means well situated to receive the reiterated frictions of the penis;" to which Dr. H. somewhat smartly demands, "how" I would "have it placed, in order so be better situated for this purpose, than it really is." "Suppose," continues he, "its situation within the pelvis is quite uncertain, is it therefore uncertain whether it is within the vagina? No, Sir, and in the venereal congress, the penis, I suppose, has no concern any where else." From the above question and remark of Dr. H. I should conclude he had never paid much attention to the subject of midwifery, or he could certainly never have confounded so grossly the vagina with the cavity of the pelvis. He would make it appear that the vagina comprehended all the space between the os externum and the uterus, let the latter be situated where it may; a limit not authorised by anatomy or physiology.

But were we to give Dr. H. all the latitude he requires for his boundaries, it would not serve his purpose; since it can be readily proven, that the os tincæ may be so situated, *even in the vagina*, that the "velvet-like head of the penis," cannot come in contact with it.

Dionis,\* regards as one cause of barrenness the deviation of the os tincæ from the centre of the vagina, as he, as well as Dr. H. supposed apposition necessary to fecundation; he says "or the internal orifice not placed directly against the external or vagina, but turned some way aside, so that the seed cannot be darted into it in a straight line, and consequently cannot get into the womb." And in p. 61, of the same chapter, in speaking of the cure of barrenness, he says it may be remedied when depending on this cause, "by ordering the woman, in the venereal act, to

\* Page 60, chap. vii.



incline to the right or left side, as the mouth of the womb is turned ; and if it fronts to the *intestinum rectum*, by using the posture of brutes.

“ Si l'orifice interne de la matrice,” says Mauriceau,\* “ est situé et regarde en dessous, ou à coté, on y pourra en quelque façon remédier, en faisant observer à la femme dans l'action du coit, une situation par laquelle la sème'nce de l'homme puisse être éjaculé vers cet orifice.”

“ The uterus has no absolutely determined situation, and takes, as we may say, a new one every instant. Sometimes it is lower or higher : sometimes it is inclined towards the sacrum or towards the pubes, and at other times to one of the sides.”—*Baudelocque*, page 156.

The deviations just spoken of, are not remarkable in the impregnated uterus, that in the unimpregnated, as one of the three obliquities, almost without exception prevails. This must necessarily carry the *os tincæ* in a direction opposite to its fundus, consequently, cannot correspond with the axis of the vagina ; this obliquity is frequently so great, that we cannot by any moderate endeavour touch the mouth of the uterus, until we have corrected the deviation, by obliging the woman to take a situation favourable to restore the fundus of the uterus to the axis of the pelvis. So extensive is this departure of the *os tincæ* from the axis of the pelvis, that in some cases the head of the child has escaped the *os externum*, covered with the uterus ;† and I am tempted to believe, that some of the cases reported, of the uterus having no *os tincæ*, and where cutting instruments have been employed to make an artificial one for the fœtus to escape through, have been nothing but extreme obliquities of this organ.

From what has just been said, I think I have made it appear, first, that the *os tincæ* has no certain location in the vagina, as it may be on either side or behind ; secondly, that in the impregnated state of the uterus, it is entirely out of the vagina, and at the superior strait of the pelvis ;

\* Liv. i, page 61.

† Baudelocque, Levret, &c.

and thirdly, that its situation in the last months of pregnancy, is so variable, that nothing but determining the nature of the obliquity of the uterus could lead us to a knowledge where it should be found; and even when that is ascertained, a great deal of trouble is frequently experienced before the finger can be made to reach it; nay, many times I have been obliged to allow the labour to advance considerably before the os tincæ could be touched.

Dr. H. p. 109, supposes the sole purpose of the projection of the os uteri into the vagina, is to receive the friction of the penis, and thus (I trust, he means) to afford pleasure; I conceive the neck of the uterus is destined for much more important objects; first, for an outlet from the uterus; secondly, as an abutment for the body and fundus when impregnated; and thirdly, to furnish all the room the fœtus enjoys after the seventh month of pregnancy. If it were for the mere business of pleasure, why does it not constantly maintain the intention of its formation, or why does its form undergo, such gradual, invariable, and important changes? Why was the business of sexual enjoyment entrusted to a thing so versatile in shape and situation?

I shall not notice Dr. H's question respecting the intention for which the os tincæ was touched "an hundred and an hundred times," for reasons I have just assigned in another place, where nearly the same object was involved; nor advert to his mode of getting over the shape of the os tincæ, for similar reasons.

Dr. H. p. 113, observes, "Although we are not bound to maintain that pleasure takes place in every prolific congress, yet we beg leave to notice the proofs against this doctrine, which the Doctor has adduced from the Abbé Spallanzani. The first is taken from the bitch; in which case impregnation was effected by means of a syringe; but was this done at a time when the bitch was indifferent, or averse to the embraces of the male? No.—Spallanzani was too well enlightened on this subject, to expect he could effect impregnation at any other time, than when the bitch

was hot." I would ask Dr. H. what proof he has, that a bitch or any other animal cannot be impregnated but when willing to receive the male? Does he know of any experiments which puts this matter out of doubt? Has it not been rather an assumed principle, that animals can only be impregnated when in season as it is termed? Does not this being in season rather prove the disposition to venery, than the capacity for fecundation? May not an animal have the capacity without the inclination? It is, I know, generally admitted, that animals conceive but at this time, and it is ascertained, that they do become prolific at this period; it was therefore chosen by Spallanzani, as being as he supposed the most favourable condition for his new experiment; but this by no means proves that no other time will do.

"But is it certain," asks Dr. H. a little farther on "that the stimulus of the injected semen produced no pleasurable sensations, especially when the genital organs were in a condition to receive with facility the impression of that stimulus?" I answer it is by no means certain that pleasure did not take place, nor is it material to the point in question, since it will go no way to strengthen Dr. H's hypothesis; for he insists, that impregnation cannot take place without the venereal orgasm, and that this cannot take place without an irritation, produced by the velvet-like head of the penis, against the extremely sensible *os tincæ*. Dr. H. therefore makes venereal orgasm as much a *sine qua non* as the male semen. Dr. H. has endeavoured to obviate this, by supposing the venereal orgasm to be always present when the animal is in heat, but as he has no proof of this, but what he derives from this being the usual season for procreation, I must not allow him this subterfuge.

Because an animal has venereal appetite, must it also have that condition termed venereal orgasm, that is so essential to fecundation, agreeably to the opinion of Dr. H.? Does he not confound here his own distinctions? In the human female, he makes it the result of irritation; in the brute, either the cause or consequence of venereal appetite.

If it be the cause of veneral appetite, it may exist independently of it ; therefore if this moment be seized, the animal may be impregnated without venereal appetite ; if it be the consequence of venereal appetite, the ultimate effect of venereal gratification arrives previous to its employment.

Dr. H. p. 115, says "but that pleasure accompanies a prolific intercourse of the sexes, the Doctor attempts farther to disprove from Spallanzani's account of the manner in which newts and frogs procreate." This is another instance of wrong quotation, or at least of wrong construction ; I have said "that with other animals, such as the dog, frog, newt, &c. on which the ingenious and accurate Spallanzani experimented, pleasure or venereal contact were not necessary," &c. To this Dr. H. observes, "I have not the work itself of Spallanzani, but in Duncan's account of it, which I have, it is stated that the 'embraces of the male begin before the exclusion of the eggs commences ; and that during the discharge of the eggs, the agitation and croaking both of the male and *female*, were very remarkable.' It is fairly deducible then, from this testimony of Spallanzani, that sensations of some kind accompany the copulations even of frogs ; and if so, the inference Dr. Deewees would wish to be made from it, is not warrantable."

I have wished no inference drawn whatever from this fact, as it came not within the range of my inquiry ; and how Dr. H. came to choose this observation, I am at a loss to determine, unless he conceived it suited his opinion, and militated against mine ; for I have expressly declared, in the experiments I had reference to, pleasure or "venereal contact, were not necessary." Dr. H. in even Duncan's account of Spallanzani, could easily have found to which of the experiments of the Italian professor, I alluded, for he says, p. 145. Comment. for 1786, "The subject of the second dissertation, is the artificial fecundation of certain animals." He then in a succinct manner, relates the Abbé's experiments on "the terrestrial toad with red eyes, and dorsal tubercles," in the following words, "he (Spallan-



zani) separated a female of this species of animal from the male, when, from the swelling of her belly, there was reason to suppose that the expulsion of the cords containing the ova would soon follow. In a few hours, after the female had been placed by herself, in a vessel full of water, the cords began to appear. When about a foot was excluded, he cut it off. He left one half in the vessel, and took out the other in order to wet it with semen he had procured from the male, that had been separated from the female, and which he had put into a watch glass. This liquor he spread with a pencil on the cord, which after this operation, was placed in a vessel of the same water as that in which the unimpregnated lay. For five days he could perceive no apparent difference between the impregnated and the unimpregnated portions. But on the sixth, he began to conceive hopes that the application of the seminal liquor had not been ineffectual. For then many of the tadpoles, in the portion which had been artificially impregnated, began to assume an elongated figure. On the seventh day, with manifest elongation, there was visible increase of bulk. On the eleventh, he perceived them moving in the amnion; and on the thirteenth, they quitted their membranes and swam about the water. On the other hand, the unimpregnated tadpoles began to corrupt, and soon turned putrid."

I have chosen to make this extract from the work which Dr. H. acknowledges to be in his possession, to prove, he either has misunderstood me, or did not read Duncan's account with sufficient care.

Dr. H. p. 117, asks if the "following would be regarded as good logic? There are a few extraordinary cases in which impregnation is said to have been effected, without the introduction of the penis into the vagina: these cases, therefore 'furnish most unequivocally the following conclusion.' The introduction of the penis into the vagina in order to impregnation, was a useless and unnecessary provision of nature."—No, it would not, but the following I think would. Cases of impregnation have occurred where

it was physically impossible, and others where the penis never did enter into the vagina ; therefore, the introduction of the penis as far as regards impregnation alone is not *essential*. In making the introduction of the penis not *essential* to generation, we mean not to deny the convenience, or the pleasure resulting from it ; pleasure we look upon as the incentive to procreation, and it has been wisely ordered it should be imperious ; it must therefore be regarded as a motive to sexual intercourse, but not an essential to impregnation.

It is pleasant to have it in our power, to obtain facts every way serving our purpose, from those who entertain sentiments very opposite to our own ; I have this gratification in reading old Dionis, who seemed never to entertain a doubt, but that the semen must be projected from the penis into the uterus, that impregnation may take place. I shall therefore, the more readily transcribe it. "I have known some *maids*, who to their sorrow, have experienced the truth of what I advance ;\* for not having permitted their gallants to enter their bodies, or emit, but betwixt their thighs only, lest they should prove with child, they thought themselves very secure ; but afterwards found, that they were *maids with big bellies, a thing not so very hard to be conceived as some imagine.*" Dionis, p. 78.

\* In order to understand to what he alludes, I will give his ideas on the subject of the semen. "The moment that a man emits, the two first drops of the seed, that is, the most subtile and spirituous parts of it, are thrown and darted into the womb, while the grosser and thicker ones move heavily along the yard, in a drivelling manner, and are lodged in the vagina." p. 78.

We need not say how chimerical this is ; being acquainted with facts which forced him to admit, that neither the penis nor semen entered the vagina, and yet the woman conceived, he was obliged to have recourse to his imagination ; this furnished him *with the two drops of semen that are first ejected.*

## INVERSION OF THE UTERUS.

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PREGNANT and parturient women are peculiarly liable to sudden and dangerous diseases; it would therefore seem a duty incumbent on every practitioner to relate whatever might tend to lessen or prevent their recurrence. The uterus is subject to such variety of derangements, that almost every day affords something new to the accoucheur. Of this kind is the partial inversion of this viscus. From the nature of the functions of this important organ before pregnancy; from the duties imposed on it by conception; from the efforts which it must exert at the ultimate period of gestation, we are obliged to regard it as one of the most interesting viscera of the female system. But unfortunately this very importance subjects it to peculiar diseases, many of which are so sudden and dangerous, that, if not instantly remedied, the patient inevitably dies.

Many cases are upon record of the complete inversion of the uterus and its protrusion from the vulva, most of which, as far as my recollection serves me, proved fatal; but no mention is made of death from its being partially inverted; and where this viscus is still confined within the cavity of the pelvis. Four cases of this kind have fallen under my notice within the last eighteen months, for the detail of which I shall make no apology, as their importance, I trust, will completely justify their promulgation, although it may not warrant my speculations on the sub-

ject; but these shall have the merit of being short, if they be not interesting.

By partial inversion I mean where the fundus of the uterus has passed either through the os externum, or is turned down inside out as far as the neck of this viscus. This takes place, I am disposed to believe, but at the full or very near the full period of gestation, as before this time the uterus is not sufficiently distended to subject it to this accident.

For the inversion to take place, several circumstances must combine: first, the body and neck of the uterus must remain flaccid, and the fundus contract after the expulsion of the child;\* and secondly, it may be essential that the placenta be ingrafted on the fundus; at least we have never seen any instance, in which the fundus, when prolapsed, was not covered with this mass.

The remote cause of this disease is, whatever may prevent the contraction of the uterus. The uterus may lose its contractile power from over-distension; this may happen from an excess of the liquor amnii, from the unusual size of the fœtus, or from compound pregnancy; it may lose this power from hæmorrhage, either before or immediately after delivery; from passions or emotions of the mind; from exhaustion by previous disease; from external violence; from its long-continued efforts to effect delivery, &c.

The proximate cause is, whatever may be capable of drawing the fundus down while the remote cause exists: this may be the placenta ingrafted at this part, or perhaps in some cases, the mere weight of the fundus itself.

The indications are simple: the reduction of the fundus, when it has not passed too far through the mouth of the uterus; and, when passed too far for restoration, to take off the stricture occasioned by the mouth through which

\* My reasons for this opinion are: first, when this disease exists, we find the tumour very firm, while the body and mouth may remain flaccid some time; and, secondly, because we sometimes find the placenta detached, which would not happen, had not the fundus contracted so as to throw it off.



it has passed contracting too forcibly on the body,\* and thus producing disturbances and consequences similar to those which arise from a portion of gut being strangulated.

The first indication is to be fulfilled by placing the back of the fingers against the tumour after the placenta is removed,† and pushing it in the direction of the axis of the uterus until the fundus is restored to its natural situation.‡ Should we find the body of the uterus too flaccid to retain the fundus in its proper place, we ought gently to stimulate it, by rubbing the fingers against it until it contract sufficiently; nor ought the hand to be withdrawn until this effect is produced. I do not know from experience that this extreme torpor may exist in the case we are speaking of, but think it possible; we shall have therefore two difficulties to contend with in this situation of the uterus, namely, a disposition in the fundus to prolapse, and, secondly, hæmorrhage, both of which will perhaps be better obviated by the irritation produced by the presence of the hand than by any other means. But, in case of alarming hæmorrhage, I would not solely depend on the presence of the hand to produce contraction, but would give the acetate of lead freely, and have a stream of cold water poured on the abdomen, and more especially by frictions with the bare hand over the region of the uterus. We have nothing to apprehend from this procedure, as it is warranted by experience in hæmorrhage, not attended with a prolapsus of the fundus.

As soon as the fundus is completely pushed up, and we perceive the uterus to contract, we may safely withdraw the hand.

The second indication is to be fulfilled by grasping the

\* See Cases III. and IV.

† In some cases, it is best not to detach the placenta, before we attempt the restoration of the fundus. This happens when the fundus has but partially passed through the os internum.

‡ By fundus, is to be understood all that portion of the uterus above the insertion of the Fallopian tubes; by body, all that portion between them and the neck.

tumour firmly, and drawing it towards the os externum pretty forcibly; by this means we make the body of the uterus pass through its mouth, which is the contracting part. This, I believe, will always be easily effected, as the prolapsed part passes from a greater to a lesser bulk, in proportion as we approach the mouth; for the uterus, as soon as emptied, will return more or less to its pear-like shape. It may be proper to observe, if this case be of any standing, and the bladder not empty, the urine should be drawn off by the catheter. See Case II.

This disease may be suspected when the following circumstances obtain: first, where we find the placenta very bulky and firm at the os externum soon after the expulsion of the child, and that it gives more than ordinary resistance to delivery when it is attempted by an exertion at the cord: secondly, when we have applied as much force as the cord ought to bear when the placenta is thus low in the vagina, and we do not find it advance; if the patient complain of much pain from this exertion; and, more especially, when we attempt to aid the force applied at the cord, by hooking the placenta with a finger, we still find it give uncommon resistance: thirdly, when the patient complains of much pain, has some hæmorrhage, is very faint, has cold sweats, and becomes extremely pale, more especially when this paleness cannot be accounted for from the quantity of discharge.\* Under these circumstances, we ought to desist from all attempts to deliver the placenta, until we examine whether the fundus be not prolapsed with this mass. For this purpose, we should either pierce the placenta with the fore finger of one hand, and tighten the cord with the other, or should search for an edge of the placenta, and trace this to the place of adhesion; if we find there a round, solid, and rather rough surface, we may be sure that the difficulty to the delivery of the placenta arises from a prolapsus of the fundus. Having ascertained the nature of the difficulties

\* See Case IV.

to be overcome, we introduce the left hand, if the patient be on her right side, and the right should she be on her left side, or either if on her back, and carefully separate the placenta by insinuating the fingers between it and the uterus: after it is separated, it may be withdrawn by the hand that is without; we then proceed, as has been already directed, to restore the fundus; or should the prolapsed part be small, we may attempt the restoration without separating the placenta.

We are generally cautioned against any violent attempts to deliver the placenta: this advice I conceive founded on just principles, but not precisely on the one for which it seems especially recommended. The only object of this admonition, with the generality of those who have urged it, is to prevent an inversion of the uterus; but this fear is founded much more in theory than in practice, in the generality of cases, as it would be necessary to this accident, that the placenta should be ingrafted on the fundus, and the uterus be in a state of atony. But experience constantly proves that the placenta is not more (nay, I may very certainly say not so frequently) attached to the fundus as to other portions of the uterus: and if it be not attached to this part, any force applied to the funis could not produce the inversion, since it could not act in the direction of the axis of the uterus, through which the fundus must fall. Indeed, I believe it may take place even after the expulsion of this mass, from the simple weight of the fundus itself. But I do not believe this accident can happen when the placenta is attached to the body of the uterus, at least while it preserves that attachment, as this mass will serve to keep up the fundus. Now, as I never would have the rule dispensed with, as the circumstances favourable to an inversion may combine, and would only require a small force in addition to the weight of the placenta to produce it, I would urge that we should not exert any force on the cord until we have ascertained, by externally examining the abdomen, that the uterus had contracted. I by no means believe a force on the cord to be absolutely ne-

cessary to produce this terrible disease, yet I can very readily conceive, that in a given case (where the atony was either not very great, or only partial) this force, other things being equal, might bring on an inversion, from which, had it not been applied, the woman might have escaped. The cases I am about to relate are, I conceive, proofs of the first of these positions; for it appeared, on strict inquiry, that no force had been applied by the midwives in cases I. II. and IV. and, as case III. was entirely under my own management, I am enabled confidently to assert, that force is not necessary to this species of prolapsus. I am the more anxious to insist on this point, that practitioners, as well male as female, may not be indiscriminately blamed when this untoward case may happen. I would farther urge, that where more than ordinary force appeared necessary to deliver the placenta, or, in other words, if the placenta, when plainly felt in the vagina, can not be delivered by the force commonly proper to be employed, that we should not persist in this force, until we had satisfactorily ascertained that the fundus was not prolapsed with the placenta.

From four cases of this disease having occurred in such quick succession, I am disposed to believe this accident to have happened more frequently than we might at first sight be willing to allow. May we not reasonably trace many of the fatal hæmorrhagies after delivery to this source? May we not attribute other instances of death to this cause, where the disease was neither sought for nor suspected? Do not some of the fatal syncopes which sometimes take place after delivery, depend on this state of the uterus?

## CASE I.

On the 2d of July, 1807, at ten o'clock, A. M. I was called to the wife of Samuel N——, in labour with her first child. Her pains were weak and irregular, but pretty frequent; presentation perfectly natural. As every thing



appeared promising, I left her to the care of her midwife. At four o'clock, P. M. she was suddenly delivered; considerable hæmorrhage with faintings followed. I was again sent for, but did not see her until six o'clock, as she lived at some distance from the city. I found her without pulse, cold, and covered with perspiration; with laborious and hurried breathing; the placenta not delivered, and the hæmorrhage continuing. I ordered her such remedies as appeared most pressingly indicated, and immediately examined her per vaginam. I found the placenta just within reach of the finger, and attempted to withdraw it, but it gave great resistance and extreme pain. I now introduced my hand, and found a tumour resembling in shape and size the indentation at the bottom of the common black bottle, over which the placenta was spread. This case was perfectly new to me, although I strongly suspected the nature of the disease. I searched for the detached portion of the placenta, from whence the flooding proceeded, and carefully detached this mass from the tumour; I then endeavoured to push up this body, but quickly desisted, from the extreme pain it occasioned, and the uncertainty that it was the best mode of proceeding to procure relief. My patient died in half an hour.

I obtained leave to inspect the body, and Dr. Rush very kindly accompanied me. It proved, as I had previously suspected, to be a partial inversion of the uterus. I dissected out the uterus, which was still so flaccid as to be turned inside out with as much facility as a soaked bladder. The fundus dipped into the body of the uterus about three inches.

#### REMARKS.

The extreme situation in which I found this patient, renders it very doubtful whether the reduction of the uterus would have been attended with any advantage, but had I had the knowledge of the disease that I now have, I cer-

tainly should have attempted it. It may appear to some who speculate on these subjects in their closets, that I failed in enterprise ; but let it be recollected the disease was perfectly new to me ; that the poor woman was absolutely in articulo mortis ; that the pain of the attempt was extreme ; and at the moment I believed, that even reduction, were it a prolapsed fundus, would be unavailing, will prove an apology for not persisting, and prevent the charge of suffering a patient to expire before my eyes, when there was a chance of relief. The death of this poor creature was more owing to the immense loss of blood, than to the prolapsus ; and the hæmorrhage must be considered as proceeding from the uncontracted state of the uterus. It may be asked how this could happen : the uterus be in a state of relaxation, sufficient to give rise to a fatal hæmorrhage, yet offer so much resistance to the resistance to the reduction of its fundus ? The answer is at hand. It is well known that the different parts of the uterus may be at one and the same time in opposite conditions ; that is, one portion may be in a state of contraction, while another may be in a state of relaxation. See Baudelocque, p. 146, vol. I. Thus, then, I conceive this case to have been ; the fundus of the uterus is never, I believe, sufficiently ample to receive the whole of the placenta, consequently portions of this mass will be attached to its body ; this part, from some cause not sufficiently obvious to mention, was at the moment of delivery in a state of atony, (or, as Baudelocque emphatically calls it, syncope ; ) the weight of the placenta dragged the fundus through the flaccid walls of the body, while the fundus retained its power of contraction ; this contraction would separate a portion of the edge of the placenta from the body, and thus expose the vessels that were before shut by its attachment ; some of these vessels are large, and will in the course of a short time pour out an immense and deadly quantity of blood. The uterus will recover its contractile power sometimes even in the moment of death. This I believe to have hap-

pened in this case, as the fundus itself was very firm, and the body, as I have already observed, gave a resistance not to be overcome by the force I used. I did not employ much power, but more than sufficient, I am certain now (from experience,) to have carried the fundus through the body had it been still flaccid. In case III, I succeeded in the reduction of the fundus with a force not greater than that employed in case I.

The uterus may regain or retain its power of contracting in the moment of death\*; it may also lose it again in this moment. This took place in this case, for at the time of opening the body it was in every part perfectly flaccid. Does this not prove that its action does not depend on elasticity, as has been asserted by some? does it not prove its muscularity?

## CASE II.

On Friday, 24th March, 1808, at half past 5 o'clock in the morning, Mrs. P. was delivered of a living child; her waters discharged themselves six or seven hours previously, and before her midwife was called. The placenta came away spontaneously, as the midwife asserted, and to which the patient herself agreed; its expulsion was attended with great pain and great flooding; she vomited severely for an hour, and several times fainted without an abatement of the discharge. This, however, was eventually moderated by the acetate of lead, and perhaps contraction of the uterus itself.

After this she continued pretty tranquil, but weak, until Sunday morning, when there was a renewal of the hæmorrhage, with pains resembling those of labour. These ceased in the afternoon; but she became more alarmingly ill. She now fainted frequently, and the discharge continued. In this way she kept until Tuesday, at which time

\* See Harvey, Baudelocque, &c.

I was called, at the desire of Dr. Atlee, whose patient she now was. The Doctor suspected the true state of this woman's case, and mentioned his opinion to me, to which at first I could scarcely assent, as almost all the cases I had ever heard or read of, as well as I recollected, had soon proved fatal; and the case I had witnessed but a few months before but served to make me doubt the Doctor's representation, or rather opinion. Here, where his judgment correct, was an instance of inverted uterus of four days' standing: a case giving contradiction to all I had heard or believed on the subject.\* I however visited the patient by appointment, and found her almost exhausted; her pulse so frequent as not to be numbered, and so small as scarcely to be perceived; great difficulty of breathing, and became faint on the least motion; insatiable thirst, frequent vomiting, cold extremities, and a continuance of uterine discharge. I examined her, and found, as Dr. Atlee had declared, the uterus to be inverted. The fundus was down at the os externum, and could readily be seen partially covered with a thin coagulum of blood when the labia were separated. The places not hid by this coagulum were rough or spongy, and of a dark brown colour.

A very dreary prospect presented itself by ascertaining this poor woman's situation; we believed death to be inevitable. But one resource offered itself, namely, to attempt the reduction of the fundus, hoping, as the uterus had not escaped from the vagina, the inversion might not be so complete as to render this impossible. We accordingly proposed this attempt to the husband and friends of our patient, candidly stating her situation, and the almost certain result if relief was not obtained in this way. They without hesitation submitted the case to our management.

We carefully drew her to the side of the bed, and had the knees drawn up and supported. I gently introduced

\* Since writing the above, I have strong reason to believe that the inversion did not take place until the morning, namely, Sunday, on which there was a renewal of the flooding, and the occurrence of pains resembling labour, as at this time the uterus suffered an universal atony.



my hand under the tumour, and gradually raised it; this gave me sufficient room to examine the nature and extent of the inversion. The instant I raised the womb there was a large and sudden discharge of urine: this gave still more freedom to an examination that was to terminate in the disappointment of my hope of the reduction of the fundus. I found so much of it had passed through the mouth of the uterus as to render any attempt at reduction futile, and the more especially as the tumour was augmented by its having swelled since its prolapsus. The stricture occasioned by the contracted mouth was readily felt, and was very strict. I was extremely perplexed for the moment how to proceed, or to announce the failure of an attempt that alone at first sight appeared to promise success or even relief, but it fortunately occurred to me, before I withdrew my hand, that I might take off the stricture by inverting the uterus completely. Agreeable to this suggestion, I grasped the tumour firmly, and drew it pretty forcibly towards me, and thus happily succeeded in slipping the remaining portion through the constricting mouth. The woman was almost instantly relieved from much of the anxiety and faintness she had before experienced; but as she was so exhausted by previous suffering and discharges, and as the internal surface of the uterus was now exposed to the influence of the external air, I was prevented from feeling or giving the slightest encouragement of recovery to her friends; but fortunately the event proved how groundless were my fears, for from this day she rapidly recovered, without another alarming or troublesome symptom.

Milk was freely secreted on the fourth day after, and continued freely. Our patient was twenty-three years of age, delicate, but always healthy, but more especially so during her pregnancy.

I visited this patient to-day, November 26, 1808, and found her at the wash-tub, perfectly well; suffers no inconvenience whatever from the uterus; menstruated regularly for three periods; had more or less discharge of mucus

tinged with blood for four months; this last four months has had no discharge of any kind; suckles her child, which is remarkably thriving. The uterus is so much contracted as to be no longer within reach of her finger.

## REMARKS.

In this case we see with what wonderful facility parts accommodate themselves to new situations; the mouth of the uterus is now within the abdomen, while the once internal surface of this viscus is subjected to the action of the external air, but whose influence it appeared to resist for some time, as it persisted for three months in the regular secretion of the menstrual blood. Nay, we do not know whether this is stopped even now by any change effected on its now external surface; it may be the natural interruption from suckling. May this woman again conceive? I do not believe it impossible. It is a case well worth watching, for should this woman again prove pregnant, it will effectually settle a long disputed point of physiology; it will incontrovertibly prove that the semen is not conveyed through the os tincæ to the cavity of the uterus, from thence to the Fallopian tubes, and from thence to the ovaria, to produce conception.\*

## CASE III.

On the 23d of November, 1808, Mrs. G—— was suddenly delivered of a large female child, which breathed and cried freely immediately after its birth. The funis

\* I was this day called to Mrs. P. (June 1, 1810,) on account of indisposition; she gave the following account of her situation: "She had been pretty regular ever since last report, but for the last few periods it has been more abundant, and is sometimes accompanied by the discharge of coagula; it continues longer than formerly, and when it ceases, it is followed by profuse fluor albus." I saw Mrs. P. again in April, 1818, and found her enjoying a very fair proportion of health, the catamenial discharges had ceased for the five last years, and has been a widow several years past; she has never been impregnated since her accident.

was not cut until after the pulsation in the cord had entirely ceased, which was in about ten minutes. After the child was taken away, I took hold of the cord, and merely tightened it, on which she begged me to wait, as it gave great pain. I, however, traced the cord to the vagina, and found at the os externum a placenta I thought unusually dense and large. On gently attempting to withdraw it, as I thought it loose in the vagina, I found uncommon resistance, which I attributed to its bulk, and desisted from farther effort, hoping the uterus would, by contracting, push it completely down. In this I was disappointed;—some hæmorrhage ensued. I now expected a more than common cause detained the placenta in the vagina, and began a more minute examination. I pierced the substance of the placenta with the fore finger of my left hand, and tightened the cord with my right; beneath the placenta I perceived a round hard substance, which I but too quickly discovered to be the fundus of the uterus inverted. I immediately introduced my hand into the vagina, and found the detached edge of the placenta from which the discharge proceeded. I carefully separated the whole of this mass, and withdrew it from the pelvis without the least difficulty. A considerable flooding ensued.

As Mrs. N——'s case (case I.) gave me a complete insight of the mechanism of this displacement of the fundus of the uterus, and as I had resolved to attempt its reduction if ever an opportunity again offered, I instantly, after withdrawing the placenta, introduced my hand, and pressed the prolapsed fundus firmly with the back of my fingers, and carried it upwards in the direction of the axis, of the uterus, and in less than half a minute succeeded completely in restoring it. Mrs. G—— has not had a single unpleasant symptom.

#### REMARKS.

The success attending this case warrants, I conceive, the hope that this formidable disease may always be re-

lieved, if means be properly used. It points out the necessity of a careful search in the vagina, where unusual difficulty attends the expulsion of the placenta; where there is hæmorrhage, and the placenta found at or near the os externum; but, above all, when great pain is felt when any force is exerted on the umbilical cord.

## CASE IV.

Mrs. G—— was delivered on the 25th December, 1808, at six o'clock, P. M. after a labour of some hours, of her first child. The placenta was extracted in about fifteen minutes without force. There was some hæmorrhage, and considerable pain. She was put to bed, and became very faint, and complained of great pain, which was occasionally augmented. She continued in this way, only gradually becoming worse, until nine o'clock, at which time I was sent for.

I found her with a small frequent pulse, great anxiety, extremely pale and cadaverous, and in a profuse cold sweat. I inquired respecting the flooding; but this did not appear to be sufficient to account for her present situation. I immediately suspected a partial inversion of the uterus, and thought proper to apprise her friends of the probable cause of her distress and danger, and of the possible result of it. Every thing was left to my management. Upon applying my hand to the abdomen, I found the uterus sunk pretty low in the pelvis, and indented at its top. I immediately after examined per vaginam, and found my conjecture but too true.

The uterus was found inverted, and its fundus was just within the os externum. I was much alarmed for my patient, as three hours or rather more had elapsed between the time of her delivery and my being called; she was much exhausted, and in extreme agony. I quickly introduced my left hand into the vagina, and applied the back of my fingers firmly against the tumour, while I moderated its



influence in carrying the uterus directly up through the pelvis, by having a gentle pressure made upon the abdomen above it. The tumour soon began to yield, and in about two minutes the fundus was completely restored.

On the third day after, my patient complained of a severe pain in the right side just above the ilium, for which I bled her freely, and purged her briskly. Nothing unpleasant supervened after this ; she might be said to have had a good getting up.

#### REMARKS.

Three hours were lost in this case, from a belief that all the pain and anxiety was owing to after pains as they are termed ; but when 120 drops of laudanum did not relieve her, the midwife became alarmed, and I was sent for. This patient would have been spared much distress had the disease been instantly known ; and the risk of death prevented, had the uterus been quickly replaced. I say risk of death, for this there certainly was, as her symptoms were as alarming as possible ; nor was there any ground from experience to hope for a reduction of the fundus, as so much time had been lost. This case I deem highly important, as it teaches us not to abandon our patient under these circumstances, and to attempt reduction at whatever time we may be called. We certainly cannot limit the time at which this attempt shall be rendered unsuccessful ; this may in some instances happen before the period of three hours, or perhaps one ; and it may be possible at even a later period. May not the disposition to syncope in this case, have retarded the contraction of the body and neck ?

It was insisted on, in this case, that no unusual force was used to deliver the placenta ; it separated from the uterus spontaneously, and was expelled without introducing a finger into the vagina.

AN ESSAY ON

## PUERPERAL CONVULSIONS.

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THERE is no disease to which a pregnant woman may be subject, so sudden or so menacing as convulsions : their attack is always ferocious, and their consequences but too often fatal.

And what renders this disease still more formidable is, the contradictory directions given for their cure. The young practitioner, instead of becoming enlightened by consulting authorities, is bewildered by the opposite opinions on this subject ; he either pursues with reprehensible temerity the directions of one, or blameably temporises agreeable to the views of a second, or fatally urges the remedies of a third.

“ But who shall decide when Doctors disagree ? ” With a view to answer this reproachful question, the present sketch of this disease is attempted. The contradictory methods of cure proposed by authors, have originated in the pathological views they took of this complaint ; and from their having but too generally supposed that there was but one species of convulsions to which a pregnant woman might be liable.

While some, as La Motte, Puzos, Osborn, &c. conceived they arose from the irritation of the uterine fibres ; others, as Hull, Garthshore, &c. believed them to be epileptic ; while Bland and some others imagined them to be nervous.

The first set make safety exclusively to consist in immediate delivery ; the second deprecate the practice, and rely for success upon the powers of nature ; while the third recommend the use, the fatal use of opium.

From these discordant views of this terrible disease, arose the various practical directions for the cure. From success having attended delivery in some instances, it was hastily believed it was the only resource ; from the want of success in other cases, it was given up with too much facility ; while in other instances more fortunate, but more rare, opium was found useful, and it was recommended with a confidence it but ill merited. From this view of the subject it would appear, that success has attended each of the methods just noticed in the hands of their respective advocates ; but that this partial good had persuaded each it was the only plan that could rationally be adopted :—than which nothing can be farther from the truth, and of course must have had many victims.

Neither of the plans of cure which have just been mentioned can be uniformly pursued without the most fatal consequences following ; it therefore shall be our business to attempt such considerations of this disease as shall remove all ambiguity from the treatment, as we conceive the subject completely susceptible of such consideration.

In prosecuting this plan, we shall first advert to the supposed causes of this disease ; second, notice the premonitory and ultimate symptoms ; third, mark the distinguishing signs of each particular species ; fourth, lay down the method of cure.

*Of the alleged causes of Puerperal Convulsions.*—The causes of convulsions are so completely hidden in obscurity, that any attempt at a theory of them, until better understood, would not only be futile and objectionable, but might be productive of serious mischief. We shall therefore merely state a few of the reputed causes, and give some occasional remarks upon them.

Dr. Denman (vol. ii. p. 405, of his Intro. to Midwifery,) says, “ it is remarkable that this disease so rarely occurs in the country,” and “ that a remote cause of it may be sought for in the particular influence of the air, or in some change made in the constitution, by the customs and manners of living in cities and large towns.” And that “ the

cases which happened out of this city (London) have happened in large towns, or among those who might be reckoned among the higher ranks of life." These remarks of Dr. Denman are by no means confirmed by what I myself have seen. In the part of the country where I once resided,\* I saw several cases in the course of between four and five years; and in this city, where I have practised more than twenty years, I have been called to many labouring under this disease, almost all of whom were of the lower class of people; and to me, the robust and plethoric appear to be much more obnoxious to this disease than the delicate and debilitated in the higher walks of life.

How far the condition of the air may be instrumental in producing convulsions I am not prepared to determine; but that one season is more productive of this disease than another, I am by no means willing to admit, but yet cannot absolutely deny. I have paid much attention to this disease ever since it first came under my observation; and perhaps no one person in this city has seen more of it than myself; and had this decidedly obtained, I think I could not have failed to have remarked it, the more especially as I kept pretty extensive records of the cases that have fallen under my notice. I wish, however, not to be understood as denying it—it must be left for farther observation.

It is said by Dr. Denman, that every part of the body becomes more irritable during pregnancy, in consequence of their sympathy with the uterus. And that this is especially the case with women whose habits of life and mode of education are of the enervating kind, and consequently they are more subject to this disease than "those women who, by education and habits of living, are seasoned, as it were, against impressions which might affect either their minds or constitutions; for it is to both these we are to look for the causes of convulsions." I have already remarked that, as far as my observations have extended, it is the robust and plethoric who are more liable to this dis-

\* Abingdon, about ten miles north from Philadelphia.



ease. And I may add that, for the most part, when the delicate and relaxed are seized with this complaint, the convulsions are generally of the hysterical kind ; which, as we shall say presently, are neither so frequent nor dangerous as the other species.

A mercurial preparation applied to the head has, according to Dr. Denman, occasioned convulsions.

He says, “ I recollect two instances of women who had convulsions at the time of labour, preceded by violent headach brought on, as it appeared, by the use of some mercurial preparation mixed with the powder used for their hair.”

It is difficult to believe that the mercury employed, as stated, was the cause of convulsions.

I have frequently known this mineral employed in the diseases of pregnant women, whose labours were neither preceded by headachs, nor followed by convulsions. It is much more reasonable to suppose, that the headachs and convulsions would have taken place had not this drug been employed.

An over distention of the bladder and rectum, has been accused of occasioning convulsions.—La Motte relates a case of the former, but I know no good authority for the latter.

The pressure of the gravid uterus upon the descending blood vessels, causing a regurgitation of blood to the upper parts of the body, and the head in particular, has been alleged as a cause of convulsions.\* But as this pressure is pretty uniform in every pregnancy, and convulsions are comparatively of rare occurrence, we can hardly be justified in assigning this as a cause of them.

“ An extreme sensibility of the uterine fibres, a violent distention of the edge of the orifice, of the uterus, and of the parts which form the entrance of the pudendum,” are considered by Baudelocque,† as causes of convulsions—this may truly be the case, but I doubt if it ever produces

\* Baudelocque, vol. ii. p. 95.

† Ibid. vol. ii. p. 94.

the kind that would, if even left to themselves, have a fatal termination.

It seems to be a fact well established, that convulsions may become periodical, and return with as much certainty and regularity as the paroxysms of an intermittent fever. Levret relates a case of this kind, where the fits recurred every day at the same hour, and toward the latter part of pregnancy they lasted eighteen hours out of the twenty-four.\*

Baudelocque† tells us of a case that returned every month during three successive pregnancies; she was attacked at the period at which the menses were wont to return.

It may also be observed, in patients subject to epilepsy, that the fits may return during pregnancy at their accustomed period, without particularly deranging the economy of gestation. I have never but once seen an instance of this kind—it was a case of true epilepsy. In this woman the fits were of pretty regular recurrence, that is, about once a month—they neither appeared augmented nor diminished by pregnancy; in two instances they seized her during labour without doing more injury to the system than ordinary; once I witnessed them immediately after delivery—they did not, in any instance that I saw, require any particular treatment from the circumstance of pregnancy—in no instance did they produce abortion or premature labour.

I am therefore disposed to believe from this case, what has been advanced by Baudelocque,‡ that when epilepsy is a constitutional disease, its attacks may be supported during pregnancy, without any manifest injury to gestation.

Having thus briefly noticed some of the reputed remote causes of this disease, I shall proceed to consider the general premonitory and ultimate symptoms.

In almost every case of convulsions during pregnancy, we may observe the attack to be preceded by the following

\* Levret sur l'Abus des regles générales, p. 15.

† Baudelocque, vol. ii. p. 96 and 97.

‡ Ibid. vol. ii. p. 94.

train of symptoms, which differ more in intensity and duration than in peculiarity.

Headach, ringing in the ears, vertigo, and often a temporary loss of vision—these symptoms continue a longer or shorter time in different patients; some complaining of them many days, others but a few hours, while others only a few minutes before the convulsive paroxysm takes place.

These symptoms so uniformly prevail before the convulsions come on, that I have almost constantly, where I have been consulted for severe headach in women near their term, directed a liberal bleeding with pretty smart purging. And by this anticipation I have no doubt but that this formidable disease has been prevented. There is a symptom which I have observed in several instances, that has never failed to be followed by convulsions, and they of the epileptic or apoplectic kind; this is a severe and intense pain in the middle of the forehead, resembling, as they expressed it, “a nail driven into the head.” So certain have I constantly been that this was the prelude to convulsions, that I have uniformly and promptly used the lancet wherever I have been permitted, and I firmly believe with the most salutary effect. In one patient who was attacked with this symptom during labour, my admonitions were unavailing—the patient obstinately refused to be bled, notwithstanding every argument I could use; nor could the anxious solicitude of her husband and friends overcome her indomitable prejudices against this operation; the result was as I predicted—she was very soon after seized with convulsions. She was instantly bled to a considerable extent—she was promptly delivered; and every means was employed that either experience or solicitude could suggest, but all was unavailing—she but too soon fell a sacrifice to the disease. I have no hesitation to give it as my decided opinion, that, had she permitted me to have drawn blood at the time I urged the necessity, that she might have escaped this terrific malady. I could not obtain leave to inspect the body after death.

Some are said to complain of violent pain in the stomach,

previous to the attack of convulsions ; this by Dr. Denman is considered a more fatal symptom than headach ; of this I can say nothing from my own experience—others have complained of pain in the back part of the head, and neck, &c.

I think I have pretty uniformly observed, that, where the headach, &c. preceded a considerable time the attack of convulsions, the disease was milder or more manageable ; while on the other hand, the severer the headach, and the shorter the period previously to the attack, the more obstinate and dangerous was the malady.

The most suddenly fatal case I ever remember to have seen, was one where the patient suddenly cried out, “ O my head, my head !” and was immediately seized with convulsions, of which she died in a few hours—this case will be related presently under the head of the “ Apoplectic Species.”

Pregnant women may be seized with convulsions from other causes than gestation. I have reason to believe this has happened frequently ; and I think we have every right to suspect this to be the case, when this complaint is unaccompanied by any symptom of labour—and if my observation be correct, they are more dangerous by far than when they proceed from pregnancy as a remote cause. When pregnancy is instrumental in the production of this disease, it is almost always at that period, when the uterine fibres are at their greatest stretch, and when the os tincæ is about to dilate for labour ; or where they suffer some peculiar irritation (over which perhaps we have no control) from the contents of the uterus which has the same effect. I have known them induced some time before the full period of gestation : but where the uterus suffered the same kind of irritation as at its full development ; for in these instances preparation was making for labour, as was proved by examination per vaginam.

As far as I have been able to ascertain, this disease is never preceded by an aura that is generally so manifest in epileptic patients. After the patient has suffered a shorter



or longer time with the symptoms we have just described, she is suddenly seized with quickly repeated spasms—the face and eyes are twitched with incredible quickness in almost every possible direction; the arms, legs, and the whole of the body are violently agitated—one side is sometimes more affected than the other; the face becomes flushed, then livid, nay black: the tongue is strongly thrust forward between the teeth, by which it is very frequently severely wounded; the respiration at first is much hurried, but eventually becomes almost suspended; the carotids beat violently; the jugular veins are distended; a peculiar noise is made by the mouth, not unlike what is termed “a cat spitting”;\* a froth issues from the mouth, for the most part tinged with blood from the lacerated tongue; the pulse in the beginning is full, frequent and tense, but quickly becomes rapid, small, and eventually almost imperceptible—the urine and fæces are sometimes discharged; a cold clammy sweat bedews the whole body, and the fit then begins to subside.

This for the most part is gradual, seldom or never ceasing suddenly and at once—the convulsive motions abate in their violence; the pulse becomes more distinct and less frequent; the breathing is less hurried and less oppressive; the face loses part of its lividity; the muscles are now agitated only at intervals, and their action resembles very much the commotion excited by the passing a brisk elective shock through them, and eventually they become entirely tranquil—the patient however for the most part remains insensible or comatose, attended by a stertorous breathing or loud snoring—she cannot be roused by any exertion for some time, and when she does recover her scattered senses, she is most generally without the slightest recollection of what has passed. This truce is, but too frequently, of but short duration; convulsion succeeds convulsion without our being able to determine with any exactitude the cause or period of their return.

\* There is something so characteristic in this noise, that Dr. Denman has declared he could tell the condition of the patient, though in another room.

When this disease attacks a woman absolutely in labour, or when this is about to take place, we may observe a pretty strict recurrence of the fits with the probable return of the pains—for although the patient be insensible to external occurrences, she appears to manifest by her moans, and that suspension of respiration that is common during a labour pain, that the contraction of the uterus has taken place; this has appeared to me to be so evidently the case, that I do not hesitate to believe that we can, by attentive observation, always tell whether this process is going on or not, without an examination per vaginam.

The face becomes very much swollen, particularly the eye-lids and lips; indeed the whole body seems to partake of this intumescence, but none so conspicuously as the face. So completely is the countenance changed, or rather disfigured, that oftentimes we could not recognise the dearest or most intimate friend; nor does this swelling immediately subside with the convulsions which caused it; it frequently remains many days after they have disappeared. Dimness of sight, nay blindness for weeks, are no infrequent consequences of this disease.

#### OF THE DIFFERENT SPECIES OF PUERPERAL CONVULSIONS.

I shall divide puerperal convulsions into three different species, that I may be the better enabled to lay down the rules which should govern our practice in these cases. I shall, therefore, for the sake of mere distinction, without attempting to defend the propriety of the terms, call the first the Epileptic, the second the Apoplectic, and the third the Hysterical species.

In the first we have always, I believe, the premonitory symptoms some days before the attack of convulsions; it is uniformly attended with a strong determination to the head, producing an engorgement of the vessels—it may come on at any period of pregnancy, but most frequently not until some time after the sixth month. This kind almost always produces labour, or at least is almost always accompanied by it, whether as cause or effect I will not

pretend to determine—but am inclined to think the former. I have seen it more than once in the same lady after the death of the child in utero.\* This kind may terminate favourably when judiciously treated, or may be converted into the second species.

In the apoplectic species we have nearly all the premonitory symptoms just enumerated, but are of much shorter duration.—It may attack, like the former, at any period of gestation, but does not necessarily, like it, produce or be accompanied by symptoms of labour. From this it would seem it may be brought on by causes independently of pregnancy, though this process may be considered as an exciting cause; for it sometimes happens, when this is at its height, and is no other ways accessory to it than by the repetition of uterine efforts, the blood is made strongly to determine to the head. It may therefore be either idiopathic or symptomatic.

In the third or hysterical kind, we have not the same train of premonitory symptoms. If headach attend, it is not so severe nor so permanent; there is frequently a ringing in the ears, and is almost always accompanied with globus hystericus and palpitation of the heart; the face is much less convulsed—the eyes vacillate much less, while the larger muscles of the body are much more powerfully agitated; the patient is sometimes very obstreperous; and the muscles on the posterior part of the body are almost always violently contracted, so much so sometimes, that I have seen a woman raised up in the middle like an arch, while her head and feet, which served to support the body, nearly touched each other. This circumstance I have considered as a very decisive mark of this species of convulsion. The face is much less flushed than in the former kinds; but I have never, to my recollection, seen it pale, as some have remarked.—There is no frothing at the mouth; and that peculiar sibilating noise which so strong-

\* The children, in the instances alluded to, evidently bore the marks of having been dead some time before the attack of convulsions, as putrefaction was pretty far advanced.

ly characterises the first species, and perhaps the second, is entirely wanting in this—the patient for the most part after the fit has subsided, can be roused to attention by a repetition of efforts for this purpose, or will frequently become coherent so soon as she recovers from the fatigue or exhaustion occasioned by her violent struggles; and though she may lay apparently stupid, she will nevertheless sometimes talk, or indistinctly mutter. After the fit has subsided a short time, she will often open her eyes, and vacantly look about, and then, as if suddenly seized by some sense of shame, will sink lower into the bed, and attempt to hide her head beneath the bed-clothes. The pulse is much less disturbed, nor does it acquire that extreme velocity and tenuity that it does in the other kinds, for respiration is never so near being suspended.

This kind attacks women of delicate habits, or those who are habitually subject to hysteria. The recovery from this is always more rapid, and never, as far as I have observed, has it left imperfect vision or blindness.

From the view which we have taken of puerperal convulsions, it will readily occur, that each particular species requires a somewhat different mode of cure; and that on the discrimination much of the woman's safety depends. We trust we have laid down with sufficient clearness the symptoms which characterise each kind, and that if due attention be paid to their respective marks, there cannot well be any gross error committed.

#### MODE OF TREATMENT.

In the first species, our great dependence must be placed upon bleeding. This must be done promptly and copiously, or no good can be expected—and one efficient mean to render this serviceable is, that the blood should be subtracted as rapidly as possible in a given time; to ensure this, large veins should be chosen, and large orifices be made. The jugular veins are opened with a decided advantage over the veins of the arms, as the blood flows more



freely, and is immediately derived from the head, the part more particularly involved in this disease. The drawing of the blood suddenly cannot be too strenuously insisted on, as I am convinced that sometimes on this circumstance alone will depend the success of the operation. It is a fact well known, that a large quantity of blood may be so gradually abstracted from the system, as scarcely to make any impression on the arterial system. When this obtains no advantage is gained, as the force of the arterial action is not weakened by the operation; nay sometimes, I am persuaded, mischief has arisen from this kind of bleeding; for the arterial system, in consequence of being relieved of part of its load when in a state of depression, acts with renewed vigour, and augments the existing mischief. The well known fact of syncope being more certainly induced by suddenly emptying the vessels, shows that the good derived from blood letting is not simply from the number of ounces that are taken from the system, but is owing to that peculiar effect which this circumstance has upon arterial vigour; for the same, or even a greater quantity allowed gradually to escape from a vein, will have no such effect.

Baudelocque\* says, "authors are not perfectly agreed upon the part where we ought to open the vein; some advise bleeding in the foot, others in the neck, but the greater part in the arm;" and adds, "it would be of great importance to fix the opinion of young practitioners on this point. I have seen convulsions of the kind described in par. 1102 (our first kind) yield to nothing but bleeding in the neck, after several bleedings in the foot; those mentioned in par. 1103 (our third kind) appear after bleeding in the foot, and be constantly removed by bleeding at the arm."

I shall merely observe on the above quoted passages, that they furnish us with the most indisputable evidence, how little the management of this disease was understood, or how little its treatment was reduced to system. It was

\* Vol. ii. p. 101.

prescribed for in the most empirical manner, and of course with the utmost uncertainty. To decide upon the part from whence the blood was to be drawn was a matter of the utmost import at that time ; a period, and in a country where the doctrine of revulsion was cherished, and implicitly believed in. At the present day, at least in this country, we are not fettered by such restraints ; we here at least know, that, *cæteris paribus*, the nearer the part affected we can draw blood, the greater will the relief afforded be ; therefore the nearer to the head that we can bleed, the more advantageous will this operation be. Dissections prove to us, that the brain invariably suffers in this complaint, to a greater or less extent. And I think we have also from these passages a strong confirmation of the opinions just advanced on the slow and sudden abstraction of blood—if these principles be not kept in view, the cases related by Baudelocque would be entirely inexplicable, but, bearing these in mind, they are of most easy solution. We shall therefore lay it down as a rule, to bleed from the jugular vein or veins whenever practicable, or from the arm or arms, from large orifices—for we must repeat, the shorter the time a given quantity of blood be drawn in, the greater will be the advantage resulting from it—hence I have in several instances bled in both arms at once.

It has been recommended from highly respectable authority, that we divide the temporal artery : I should not approve of this practice in the commencement of the disease, as a sufficient quantity of blood, in no instance in which I have tried it, could be drawn. I do not even recollect being able to obtain more than eight or ten ounces of blood by this operation, a quantity totally inadequate to the exigencies of the patient. When blood has been freely drawn previously, and more is still wanting, then opening the temporal artery may be highly advantageous, and I would recommend it as a substitute in some instances for cupping or leeching—but this operation I think should never be relied on, where much blood is necessary to be drawn. Could we by any contrivance obtain the required

quantity, and in a short space of time, this mode of bleeding would of all others be the most eligible.

When blood is drawn from the foot, it usually flows very slowly—it does not remove the plethora that is oppressing the system, but rather promotes the vigour of the arteries; hence the injury done in the cases just related from Baudelocque, when the bleeding was performed in the foot; and hence the relief derived by bleeding from the arm, as the blood flowed from this place more copiously.—Besides, the feet are so remote from the head, that it would require an immense quantity of blood to be drawn from them, before its influence could be perceived or felt in the head.

Topical bleedings by cupping and leeching should never be had recourse to in the beginning of this disease, for the reasons we have just urged against the gradual subtraction of blood.

If it be asked, what quantity of blood should be drawn in any given case, I answer I do not know by ounces—I bleed until I abate the severity of the fits, or until I arrest their repetition.—This may be effected sometimes by thirty or forty ounces suddenly drawn, but it may require upwards of an hundred in the course of a few hours. Beside bleeding generally and topically, other evacuations are to be promoted, such as purging, enemata of stimulating ingredients, sinapisms, blisters, &c.

During the paroxysm, it is no unusual thing for anxious bystanders to attempt an alleviation of them by holding stimulating substances to the nose of the patient—for this purpose, hartshorn, brandy, burnt feathers, &c. are used; this practice is seriously to be forbidden.

The dashing of cold water has appeared to be of temporary use.—I have never witnessed its influence, to the extent mentioned by Dr. Denman, nor should I ever place any other reliance on it but as an auxiliary.

With a view to exhibit the routine of practice in this disease, I shall subjoin cases, selected pretty much at random from many others of similar import.

## CASE I.

Mrs. ———, a delicate small woman, twenty-three years of age, pregnant with her first child, was attacked on the 16th of November, 1809, at 8 o'clock A. M. with epileptic convulsions. I saw her an hour after the attack—previously to my seeing her she had had three fits, and a fourth was coming on just as I entered the door. Three or four days previously to the attack she complained of a violent jaw or toothach which was looked upon as rheumatic, and no attention was paid to it. On the 15th, that is, the day before her illness she was seized with an extremely acute headach; and during the night, and, just before the onset of the fits, she was extremely sick at stomach, and vomited a large quantity of thick glairy mucus; immediately after this she said she could not see, and was, in a few minutes more seized with convulsions. She laboured under the appearances described as the ultimate symptoms, in a violent degree. I instantly bled her from a large orifice in the arm  $\text{zxxxv}$ . by measure—this, as I have just said, was at 9 o'clock A. M. Eleven o'clock, had two fits during my absence, and was now in the third—bled  $\text{zxii}$ .—ordered a strong infusion of senna as an enema—os tincæ a little opened, but rigid. One o'clock P. M. had two fits since last visit—injection operated—pulse still active—face flushed—very restless and uneasy, arising as I believed, from the pains in the uterus—os tincæ rather more dilated—to be bled by cups  $\text{zx}$ . Four o'clock P. M. one fit; cups drew well; senna operated again two or three times; very comatose—ordered cold applications to the head by means of a large bladder partly filled with water and some ice—blisters to the legs. Seven o'clock P. M. no fit since last visit—pulse very active—very restless, constantly making efforts to get out of bed\*—os tincæ not

\* I have considered this as a pretty certain sign of labour going on.



much more dilated : took  $\frac{3}{4}$ x. blood—senna continued to operate. Ten o'clock P. M. no fit since last visit—pulse still too active—took  $\frac{3}{4}$ x. more of blood—cold applications. 17th, Mr. Purnell, now Dr. Purnell, one of my pupils, staid all night with the patient. He said she had one fit, after which he took  $\frac{3}{4}$ x. of blood—senna continued to operate. At 10 o'clock A. M. I saw her—stupor much less—recognised her friends, and asked some questions—she did not see well, a slight squinting was observable. Seven o'clock P. M. better, pulse less active—had had three stools since the morning visit. 18th. Mr. de la Motha, now Dr. de la Motha, another of my pupils, staid with the patient last night. She passed a good night, was tranquil and rational—no return of fits during the night—two stools. Saw her at 10 o'clock, A. M. skin dry and hot, face a little swelled, but perfectly collected. Eight o'clock P. M. face more swollen, and a little flushed ; much headach—pulse very active—great thirst—took  $\frac{3}{4}$ x. of blood, much relieved by it ; pulse softened, and diminished in frequency—cold applications continued. 19th, passed a good night ; free from fever and pain—no return of convulsions—bowels rather tardy ; ordered senna tea. Continued much in this condition until 28th, twelve days from the first attack—this morning was seized with brisk labour pains, and was soon delivered of a dead child. From the degree of putridity, it is presumable the child died early in or before the attack.

On this case it may be proper to remark, 1st. That the child had not been felt to move for several days before the patient was taken ill—but this is by no means a certain proof of its death. 2d. Signs of labour were manifested on the first day of the illness, but was evidently suspended, or at least not progressive, after the second. I occasionally examined for several days, but found the os tincæ so rigid, as to preclude the idea of manual assistance ; it was therefore not attempted. 3d. Had manual aid been resorted to, I have no hesitation in believing it would have been extremely injurious. 4th. That the convulsions were con-

trolled ten days before delivery took place, although from the state of the uterus it was evident it would sooner or later take place. 5th. That the attack commenced between the seventh and eighth month of pregnancy. This lady's next pregnancy was not attended by this untoward accident; strict attention was paid her during the whole period of gestation. She was kept on a milk and vegetable diet—her bowels were kept open—She was occasionally bloodied, especially when she complained of headach—she took for several months three or four doses daily of the tincture of foxglove, with I think, evident advantage, and was at the proper time happily delivered of a fine child. In her third pregnancy she paid much less attention to herself, and was not under medical restraint, in consequence of which she was again attacked by convulsions, and was held very much as above related, with this exception that labour was much more rapid. Her fourth pregnancy was again fortunate, as she again submitted to medical directions. Her fifth pregnancy I have understood was again unfortunate, and attended with convulsions like two of the former—I did not see her in this last pregnancy, having been absent in the country when it happened.

## CASE II.

Mrs. —, aged twenty-six years, pregnant of her first child—a large plethoric robust woman, was on the 9th of Sept. 1811, at about 5 o'clock, A. M. taken with labour-pains, and sent for her midwife; before the midwife arrived she was seized with terrible convulsions, and I was immediately sent for—the fits were very frequently repeated, and were from their extreme violence very threatening—her face was immediately swelled—her eyes fairly protruded from their sockets—her tongue terribly wounded, &c. &c. I instantly bled her from the jugular vein more than three pints of blood—examined her, and found labour approaching—ordered a brisk injection—saw her two

hours after—had had several severe fits—pulse extremely active—labour advancing—bled her twenty ounces—injection repeated—a stream of cold water was poured on her head during the interval of the fits—11 o'clock A. M. fits not so severe, but pretty frequent—pulse still very active—took a quart of blood—apparently much relieved—lay quieter—1 o'clock P. M. had had two or three fits—very restless—mourned every few minutes, desirous of getting from the bed—bled her 3xii.—examined and found the head low in the pelvis, and delivered with the forceps—she had two or three fits after delivery—and remained insensible to every thing for forty-eight hours—she now began to show some signs of returning sensibility—was bled twice in that interval—cold was applied to the head, and the legs blistered—she was purged freely by senna tea—after this she gradually recovered her senses. She was left completely blind for two weeks—she then began to see imperfectly, but was six weeks before she could distinctly discern objects. It may not be amiss to observe the child was living.

This case is remarkable on account of the severity of the disease, and the large quantity of blood that was drawn in the short period of a few hours. She lost, in the first six or seven hours of her illness, one hundred and twenty ounces of her blood, and about 140 ounces altogether; a quantity that might, at first sight, startle the timid or inexperienced practitioner; but when he reflects, that here was a patient labouring under one of the most ferocious complaints in the whole catalogue of human diseases; the brain threatened with immediate destruction; the patient of prodigiously full habit; one who not only neglected the kindly warnings of headach, giddiness, and occasionally loss of vision, by not having recourse to bleeding—but, contrary to the advice of her midwife, fed freely and remained long costive—what then could avert the threatening consequences of this disease but the most prompt and the most subduing remedies? Had not the bleeding been carried to the extent it was, I really believe it would have been

unavailing. Even as it was, it did not prevent temporary blindness. Her second pregnancy was not attended with any untoward circumstance.

#### TREATMENT IN THE APOPLECTIC SPECIES.

We know no difference in the treatment of this disease that is very important, except that blood-letting should if possible be more promptly employed, and more extensively used than in the former—for if an hour be lost, the patient's doom may be sealed. And it must not be disguised, that the patient but too often falls a victim to its violence, notwithstanding “all the pliances and means to boot.” And we may add, as it forms a distinction between these two species, that there is one state of the patient in which artificial delivery is not to be thought of. But as this is a matter which should be clearly understood, we will state the treatment more at length. In order to do this with the least possible ambiguity, we shall divide this species into two varieties, and for want of better terms shall call one idiopathic, and the other symptomatic. By the former we wish to be understood that attack of convulsions in which pregnancy or labour has no agency in the production of: and by the latter that attack of convulsions which happens during the progress of labour, but in which this process had no other agency than producing a strong determination to the head. In variety first, we have seen a disease seize a pregnant woman without this state contributing especially to it; for if the same plethoric condition of the blood vessels should be produced without the circumstance of pregnancy, the same result would follow. In this variety then we shall find, that the premonitory symptoms preceded the attack but a short time; that they were more intense; but that the convulsions are perhaps less severe, but more obstinate in their continuance, and less regular in their return—the breathing is more strongly stertorous, or is rather a loud snoring—that there is no change made in the os tincæ, nor any evidence of uterine contraction—in a word, not a symptom of labour. Here, then, should we attempt deli-



very by forcing the mouth of the uterus, as some direct, we should inevitably destroy our patient; delivery in this case is not to be thought of, because there is no effort of nature for this object; and where this effort does not manifest itself, it were madness, nay, I had like to have said, *murderous* to attempt it. Our whole duty in this case consists in proper medical treatment, and differs in no way from that we have already suggested for species the first, except, in this case, the remedies require a more prompt and a more extensive application. Effusion but too often takes place, and all our hopes are blasted in a moment.—We may here observe, once for all, that the rules for the delivery of a patient labouring under convulsions are simple, clear, and void of all ambiguity—they are these: When there is an evident disposition in the uterus, to effect the expulsion of its contents, it is then, and then only, we are to attempt to assist it.

2. That this assistance must be given to the efforts of nature with the least possible violence.

3. That unless the labour be far advanced, and the delivery can be very promptly effected either by turning or the forceps, it should not be attempted until we have lessened the danger of a fatal effusion by a copious bleeding.

4. That no attempt should be made to dilate the mouth of the uterus when at all rigid, until we have removed, or very much lessened the determination to the head by a *sufficient loss of blood*.

5. That this *sufficient loss of blood* is only manifested by a cessation, or a great abatement of the convulsions, or by an easy dilatibility of the os tincæ.

6. That when the former condition obtains, we may safely trust to the efforts of nature to effect the latter, but if it be accompanied by the latter, the more speedily we deliver the patient the better.

7. That turning is the means to be employed when the child is still enveloped in the uterus; but when the head has escaped from this viscus, we must employ the forceps.

## CASE III.

Mrs. —, Nov. 10th, 1797—pregnant with her second child, and in the 8th month, was seized while at the ironing table with vertigo.\*—She fell, and was immediately attacked with convulsions. I was living near her, and was instantly sent for—I found her labouring under the general symptoms of this disease—I bled her from both arms at once, and took from each arm nearly, if not quite, a quart of blood. She appeared for a short much relieved; that is, the convulsions were abated—I examined her, but found no change in the os tinæ. An injection was ordered, which operated well—about an hour after the bleeding her pulse rose very much; her breathing was more laborious and stertorous, and some convulsive twitchings played over the whole body.—She was entirely insensible to all external impressions—the pupils of the eyes were much dilated; fearing a violent repetition of the convulsions, I again tied up the arms, and took about twenty-five ounces more of blood—this seemed again to moderate the symptoms—no change in the os tinæ. 4 o'clock, P. M. three hours after the attack, the convulsions were renewed with considerable violence.—She was let blood to the amount of twenty ounces—cold water was poured on the head—she was again more tranquil, but not less comatose, though the breathing was less loud; she had a copious black stool. 6 o'clock P. M. had had several fits, but not as violent as at first—pulse still too active; took eighteen or twenty ounces of blood from the arm—as the pulse was now considerably reduced, applied a pair of blisters to the legs and sinapisms to the feet. 10 o'clock, no convulsions since last visit, breathing freer, but loud—swallowed a little water with some difficulty—passed no water since the attack, introduced the catheter, and drew off a large quantity—had two stools—made an

\* She had complained all the morning of intense headach, and several times said she could not see—she was advised to leave off work, but would not.

effort to vomit. 11th, 6 o'clock, A. M. was called to her suddenly by request, as her breathing was becoming more laborious and loud, and face more flushed, with some convulsive agitations; pulse rather too active, had ten ounces of blood by cups, and a large blister between the shoulders. From this time there was no return of convulsions.—She gradually recovered her recollection, but remained until some time after her delivery, (which took place at the regular time, and with a living child) with imperfect vision, especially in one eye. She was for many years after this subject to violent headaches, which were relieved constantly by bleeding.—She had several children after this attack without convulsions.

#### CASE IV.

Mrs.—, October, 1, 1803, had been in labour several hours; she had every appearance of being happily delivered of the fifth child, when, during a strong pain, she instantly cried out “my head,” “my head,” and immediately fell into convulsions. She was under the care of another physician, to whose aid I was instantly called by his own desire—the convulsions were strong and very frequently repeated—she was largely bled; on examination the child was found to be far advanced, and was speedily delivered by the aid of the forceps—the convulsions, however, continued in spite of every exertion to relieve them, and she died in about three or four hours from the attack. Leave was obtained to open the body; the longitudinal sinus of the dura mater contained (by estimate) between two and three ounces of blood; the posterior left ventricle was filled with a bloody serum—the other ventricles appeared sound, as did the other parts of the brain—no other part was examined.

## CASE V.

Mrs. —, aged 24, pregnant for the first time, was taken in labour on 10th March, 1797—her labour proceeded regularly, and the child's head was at the inferior strait, and every rational expectation was entertained of a speedy delivery, when she suddenly cried out with pain in her head, and declared she could see no one in the room; these symptoms had continued but a few minutes when she was seized with convulsions—she was under the care of the late Dr. Shippen, who requested that I might be sent for, and desired I would bring my forceps with me. I found the patient in a strong fit, with her face literally as black as a negro—it was agreed she should be bled extensively—this was done from the left jugular vein to the amount of nearly two quarts; it had an immediate effect in tranquilising her. I now examined her and found the head low in the pelvis; I applied the forceps, and delivered her of a dead child. Upon examination it was found there was another child; the uterus soon discovered a disposition to act; but fearing injury from delay, I immediately delivered by the feet, which were the presenting parts; the child was healthy and did well; the mother had no return of fits, and she rapidly recovered her usual health, excepting that of her eye sight, which did not return so as to discern objects for several days; and her vision was very feeble for several weeks.

## HYSTERICAL CONVULSIONS.

This species is much more rare than those we have already mentioned, not occurring once for the others twenty times—it is also much less mischievous. It may occur at any period of pregnancy, without necessarily deranging its economy. I have known it repeated six or eight times in the same pregnancy without manifest injury. It is not preceded by the same train of premonitory symptoms as



the other two species—for the most part we find the patient complain of headach and especially on the top of the head ; some giddiness, and ringing in the ears, a sense of suffocation, making deep inspirations and begging for air, declaring she is so hot she shall die if she does not get air—palpitation of the heart and a rising in the throat—she is presently afterwards seized with a convulsive paroxysm, which differs in its appearance, as we have already noticed, from the other species. For the most part the convulsions continue longer, but are evidently not so threatening—it sometimes ceases suddenly, and the patient will frequently recover her senses and anxiously inquire where she has been, or what is the matter. Sometimes the paroxysm is attended by laughing or crying—when this happens, there is no mistaking the disease. It may take place during labour, but this is rare. When it happens before labour it does not appear to have any agency in producing it. I have never seen this species followed by loss of sight or permanent headachs.

Its cure is much more simple and certain than the other kinds, requiring precisely the same treatment as when this disease happens to women not pregnant. I have never considered delivery essential to the welfare of the patient, unless it attacks when this process is pretty far advanced, and seems to arise from the irritation given by the head of the child suddenly distending its mouth ; in this case, to deliver may be important, as it immediately removes the cause of the convulsions.

This form of the disease rarely requires more than one bleeding, and that not very large. After we have taken away blood, which should always be done when the pulse is full and tense, we may safely exhibit opium with assa-fœtida, which will generally pretty speedily arrest the disease. I have never found cupping or blistering necessary ; having the bowels opened by injections is important ; and enemata are often the best, and sometimes the only method of conveying the remedies just mentioned. It was the treatment for this form of the disease which

has given rise to almost all the errors in the management of puerperal convulsions; it was successfully treated by opium and antispasmodics; and as every other species was confounded with this, the same plan was pursued with them to the inevitable destruction of the patient.

### CASE VI.

I was called on the 10th of July, 1811, to Mrs. —, who was, at the moment of my arrival, and had been for a considerable time before, in a strong convulsive paroxysm. I found several men diligently employed in holding her and opposing her motions; she was raised in the middle like an arch, while her feet and head nearly met. She was between seven and eight months pregnant, and subject to hysterical affections.—She was thrown into this by some altercation with one of her neighbours—cold water was dashed in her face, and she was bled to the amount of sixteen ounces. The spasms began to give way soon after, and in the course of about fifteen minutes ceased. She sighed very deeply, and struck her arms very forcibly against the bed, and in a few minutes more inquired what all these men were doing with her. I gave her fifty drops of laudanum and two tea spoonfuls of the tincture of assafoetida in some sweetened water, and she had no return of the fit.—She went her full time without a repetition of them, and was safely delivered of a healthy child.

I shall now subjoin two other cases, to show of how much consequence a proper distinction is in the treatment of puerperal convulsions.

### CASE VII.

I was called on the 16th of April, 1810, to Mrs. —, said to be in strong convulsions. I was from home when the messenger arrived, but went so soon as it was in my

power. When I went into the sick chamber, I found Dr. — with the patient. He told me “Mrs. — had been attacked about two hours before with convulsions, and was in the ninth month of pregnancy—that previously to the attack of the fits, she had complained of violent pain in the forehead, which she told her husband she could cover with her finger.—She had this pain several days, but was much more intense this morning, and was attended with a sensation as if a piece of black gauze was before her face. She was stooping for some time over a trunk in which she was arranging some articles, when she was seized, and fell on the floor in strong convulsions.”

She was now lying senseless and without motion on the bed; she breathed very heavily and snored loudly—her face much swoln and of a purple hue—the pulse frequent and small, and the extremities cold. I inquired what had been done, and was informed by Dr. — he had given her twice, sixty drops of laudanum at a time, and that since the last dose she had had no fit, and was in his opinion very much better, requiring nothing but sleep to restore her.—I told him very plainly that I thought he had mistaken the patient's case, and had in my opinion sealed her fate by the use of the laudanum.—He appeared alarmed, but not altogether convinced.—We did every thing that we thought might be useful, but all exertion was unavailing, and the patient died in about three hours.—I could not procure leave to open the body.

## CASE VIII.

Above three months after the above event had taken place, viz. on the 20th of July, 1810, the same gentleman was called to Mrs. —, labouring under convulsions. I was sent for at his request. Before I arrived he had bled the patient very freely (forty ounces) by which she was considerably relieved—she was near her full time of gestation. From her peculiar motions and breathing, I suspected labour had commenced—she was examined, and

the os tinæ was dilated to about the size of a dollar. It was however pretty rigid. The convulsions however returned with considerable force; the patient was again bled about thirty ounces; a stimulating injection was thrown up the rectum, which operated freely: the mouth of the uterus was now well dilated; I turned and delivered a living child. Mrs. —, had one fit after delivery, but it was not severe. She recovered her senses and feelings on the second day after delivery, and no other inconvenience was experienced, except some dimness of sight and slight headach. Several days before the attack of convulsions, she had complained of the headach, and that particular sensation of a nail being driven into the head, and also of an occasional loss of sight.

The two cases just related form a happy contrast in the mode of treatment; the first case proving so unfortunate, made a strong impression on the mind of Dr. —, who very properly profited by it in the second case. He candidly confessed they were as similar as any two cases of disease could be, and declared himself much shocked at the reflections which the unfortunate case gave rise to.

In every case of convulsions, it is but too common for bystanders to oppose by strength the contractions of the agitated muscles. This practice cannot be too severely reprehended, as it very injurious and most unnecessary; it subjects the patient to severe muscular pains, which last for very many days after the fits subside.—All that should be done in such a case is, to prevent the patient doing herself mischief, or prevent her throwing herself from the bed; a very moderate exertion is sufficient for this purpose, therefore violence should never be employed.

—Si quid novisti rectius istis  
Candidus imperti, si non his utere mecum.



## OBSERVATIONS

ON MR. FOGO'S PAPER ON THE

## IMPORTANCE OF THE UTERUS.

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THE utility of periodical publications is universally admitted ; they afford an easy and certain mode of communication between men of general literature and science, and are the vehicles of many useful improvements and important discoveries ; and this applies in a particular manner to the art of healing. By their means, also, we are put in possession of practical improvements without the parade of system, and obtain by their agency useful facts that would have laid dormant with their discoverers. But may we not also add, that by their means many *false facts* are promulgated, and many speculations ingeniously defended, to the decided injury of medical science ?

We have been led to these reflections, by reading a few days since, Mr. Fogo's paper "On the Degree of Importance which should be attached to the Functions of the Uterus in regard to Health," inserted in the 6th vol. of the Edin. Med. and Surg. Journal, p. 175. And as we differ in sentiment with the author, we have taken the liberty of making some observations on it.

Mr. F. begins by observing, "I have always differed in opinion with medical writers on the subject of the very great importance of the functions of the uterus. That simple, passive, accommodating organ, has had more importance attached to it, than has fallen to the share of all the other organs taken together. That important organ,

that important secretion, that important discharge, that important time, when an important change takes place in the system, are high sounding expressions, which stun the ears, and stare us in the face in every page of the writings of physicians, when on this subject. The simplicity of its structure being known to every anatomical reader, need not be pointed out. It is also a very passive organ. When the balance between the solids and fluids is well adjusted, when the female is in good health, at a certain age, a small quantity of blood is effused from the dilated extremities of the exhalent vessels, which returns at periodical times. This merely shows, that the evolution of the organ is complete, and that the female is capable of being impregnated, and producing one of her species. The same circumstance, in an inferior degree, takes place in every female animal we are acquainted with. If the fluids are deficient, the exhalents allow none to be effused. Did this effusion issue from complicated bodies called glands, it would have been entitled to more important consideration. It cannot be called a secretion; there is no alteration made in the fluid, as happens in all the organs called glands. It is a simple effusion of real blood, unchanged. Secretions from the various glands are very different from the blood which enters them; some insipid, some bitter, &c. But how these secretions are effected, the microscopic eye of the anatomist has never been able to detect. It also differs from glandular secretions. While the female is in health, the various secretions, and this effusion, go on; but, on the system being reduced by disease, the glandular secretions continue, though in small quantity; this effusion stops, nor can it be restored until the system is replenished."

We have quoted the whole of this paragraph, that Mr. Fogo's opinions might be seen at full length. From this it would appear his ideas of the physiology of the uterus are very limited, and more especially of the production of the menstrual discharge. He looks upon this evacuation to be nothing but an effusion of "real blood, unchanged," "from the dilated extremities of the exhalent vessels;"

but admits that, were "this effusion from complicated bodies called glands, it would be entitled to more important consideration." Here Mr. F. takes for granted, what remains to be proved; and to this proof we challenge him, that the menstrual fluid is a mere evacuation of "real blood, unchanged." We are of opinion, that he cannot prove it, for the following considerations: 1st. It differs, from real, unchanged blood, in smell; 2d, in colour; 3d, in never coagulating or separating into parts, however long kept, as is proved by instances of imperforate hymen.\* Here, then, we perceive very obvious differences between the menstrual fluid, and "real blood, unchanged." This, perhaps, might be thought sufficient to convict Mr. F. of error; but we shall not rest satisfied here, but proceed to examine his opinions a little more at length.

Has Mr. F. ascertained, by any direct or indirect experiments, that the menstrual fluid is "real blood, unchanged?" If he has, he has not mentioned them; on what, then, does he ground the opinion that it is "real blood?" This he does not tell us:—It is, therefore, in our opinion, an assumed datum; and we must strongly insist on this, since we have pointed out obvious and remarkable differences; and would now ask, if this discharge were "real blood, unchanged," why does it not exhibit the marks of "real blood, unchanged?" Why does it not exhibit the phenomena that blood always does, when at rest, after having escaped from its vessels? Does it coagulate or separate, like "real blood" when extravasated, either when exposed to the influence of air, or when excluded from it? If coagula appear during the menstrual period, they are always looked upon as evidencing a state of disease, and they uniformly with us, as well as many

\* In cases of this kind, in most instances, the blood has been accumulating for a number of months; indeed, almost always until the pain arising from distention forces the girl to seek relief. Now, in every case of this kind, the menstrual blood is found to flow without the least mixture of coagula, so soon as a puncture has set it at liberty. The only change witnessed, is, perhaps, a little thickening, from the absorption of some of its finer parts.

others,\* are made the basis of an important diagnosis when consulted for profluvium mensium. For when no coagula accompany this discharge, we rarely find it necessary to interfere with it, as it but very seldom is carried to excess ; but on the contrary, where there is a discharge of coagula, we uniformly prescribe for the hæmorrhage, as we look upon this as disease. And what decidedly proves the difference between these two fluids is, that the hæmorrhage, or discharge of "real blood," shall, by suitable remedies, be made to cease, without interrupting, in the smallest degree, the true menstrual flux.

Mr. F. confesses, that, "did this effusion issue from complicated bodies called glands, it would have been entitled to more important considerations." We should have been glad to have known Mr. F's definition of a gland, since he seems to deny that the membrane which lines the internal surface of the womb has the structure. But has he proved there are no glands for the purpose of this secretion, or that the mucus membrane of the uterus is incompetent to this end? Certainly he 'as not in the paper before us—he merely roundly asserts, that "it cannot be called a secretion ; there is no alteration made in the fluid, as happens in all the organs called glands." Is this argument or bare assertion? Is this not the point to be proved ; for again we must ask, what evidence has Mr. F. that the menstrual discharge and "real blood," are one and the same fluid? We must again declare that he has none, or at least he has given us none. Why does he doubt this to be a secretion? Is it necessary to every kind of secretion that there should be "complicated bodies?" Certainly it is not. For in the most simple with which we are acquainted, the fluids undergo some "change ;" even arterial exhalation has its modifications ; without which, the elimination could not take place ; for without some peculiar action of the membranes, the serum would remain united with the other constituent parts of

\* See La Motte, Mauriceau, &c.



the blood, as it is too warm and too much agitated to permit of a spontaneous separation of its parts.

When we advance a step higher in this process, we find the mucous membranes secreting a substance which required more elaboration, and consequently a greater complication of vessels ; the arrangement which is necessary for the secretion of mucus has been termed *cryptæ*, glandular follicles, and mucous lacunæ ; and of this kind we rank the internal surface of the uterus, as it will not be disputed that there is present in the cavity of this viscus a constant supply of a tenacious fluid, designed for the use of this organ. And should it be insisted on, that the uterus, as a whole, is not a glandular body, yet it will and must be admitted, that its inner surface performs the office of secretion, and that sometimes to a great extent, and this will be sufficient for our purpose ; for it is to this surface we refer the secretion of the menstrual fluid. And perhaps we do not hazard much, when we say it is more glandular than some are willing to allow, since it is particularly obnoxious to the diseases of glandular parts, namely, *scirrhi* and cancer.

As Mr. F. commences his paper with the declaration, that "he always differed in opinion with medical writers, on the subject of the very great importance of the functions of the uterus," it would be but reasonable he should have stated fairly his reasons for this dissent ; instead of which he offers us nothing but a tissue of declamation, neither easy to unravel nor comprehend. As he would seem to rest his great argument on the bare assertion, that the menstrual discharge is nothing but an exhalation of "real blood, unchanged," we shall endeavour to strengthen what has just been suggested in favour of this discharge being truly a secretion, by an appeal to authors of the greatest respectability ; and to those who, to say the least, have had as great opportunities to investigate this subject as the author of the paper now under consideration ; and what is still more in point, some of whom entertained the

same doctrine as Mr. F. himself, yet were rather doubtful of its truth.

The first we shall notice, with this view, is Mauriceau. He declares himself pretty much of the opinion of Hippocrates on this subject: "Procedit autem sanguis velut à victima, cito congelatur, si sana fuerit mulier," but remarks, "si on remarque *quelque alteration en sa substance et en sa couleur*, cela ne procede que du melange de quelques excretions de la matrice." Here we cannot fail to remark the influence of authority: it was at this period a proof of ignorance, nay, criminal, to differ from Hippocrates or Galen, and to reconcile this slavish adherence, to the fact, he attempts to account for the alteration in colour and substance, to its admixture with some foreign body in its passage through the uterus and vagina; we need not stop to disprove this absurdity, as it carries its own refutation with it.

The next is La Motte. He says, "Je ne sais point aussi une règle générale de la bonne qualité qu' Hippocrate donne à ce sang, non plus de la mauvaise et pernicieuse que Pline lui attribue. Hippocrate dit que ce sang est semblable à celui d' une victime, et se caille promptement, si la femme est saine. Il faudroit, pour faire cette remarque, que ce sang vint comme une belle et large saignée du bras bien jaillissante. Car s'il ne venoit que comme un filet, ou goutte à goutte, il cailleroit infailliblement, comme fait pour l' ordinaire celui qui vint par la saignée du bras de cette sorte." He now adds with his accustomed naïveté and honesty, "Or le sang menstrual ne venant jamais si abondamment que la plus mauvaise saignée du bras, *comment ne cailleroit-il pas?* Et s'il vient autrement, ne doit-il pas changer le nom de flux menstrual en celui d' une *vraie perte de sang?*"

Dr. Denman says, "the menstruous discharge has been considered simply as blood, though of a kind different from the general mass, as it has been observed not to coagulate. All discharges of blood, in which there were *coagula*, have therefore been distinguished from menstua-

tion, and assigned to some other cause. Whether menstruation ought to be esteemed a secretion, similar to that made by other glands of the body, and does not coagulate because it is essentially different from blood, *which I believe*; whether the coagulation is prevented by a mixture with the discharge from the mucous glands; or whether it is a secretion from the uterus, peculiar to that part, without analogy or resemblance to that of any other part, may be proved by future observations and experiments."

Burns says, "the discharge appears to be yielded by the uterine arteries, but it is not an extravasation or hæmorrhage, for when collected, it does not separate into the same parts with blood, neither does it coagulate."

It would be easy to multiply authorities to the same effect, but we trust sufficient have been produced to prove, that the menstrual fluid is not "pure blood, unchanged." Now, if it be changed, we contend it must have suffered this alteration by the action of living vessels, and that this change is the consequence of that process termed secretion, since no other can satisfactorily be pointed out to produce this end; and as the onus probandi will lie with Mr. F. we shall rest this part of our cause on what has already been said.

Mr. F. says, "that the menstrual discharge differs from glandular secretions." What Mr. F. may precisely mean by this assertion is, perhaps, difficult to determine. If he mean by this to prove, that because the menstrual fluid resembles no other glandular secretion, that it is no secretion,\* he should have furnished us with his reasons for this conclusion, and not left us to rely on this sorry logic, and his mere assertion. It was incumbent upon him to prove what he has merely conjectured; he should have left no doubt of their identity. To have proved this, he should have made the analyses of secreted fluids from the most simple to the most compound; he should have an-

\* By this kind of reasoning, it would be easy to prove there is no such process in the human body as secretion; for if the products of this process are necessarily to resemble each other, neither bile, urine, semen, nor the gastric liquor is a secreted fluid, as there is not the slightest analogy between them.

analysed the blood and the menstrual fluid, and then, by these analyses, he should have demonstrated that there was no difference of result between these latter fluids, and very obvious ones between them and the secreted fluids. This would have put the matter to rest, and he might then safely have declared that the menstrual blood is "real blood, unchanged;" but until this is effected, we cannot be contented with this unqualified declaration, nor with his other, that "it differs from glandular secretions," especially as it would appear he wishes by this to prove the menstrual fluid to be no secretion.

We are of opinion that something more than unqualified assertion is expected from a man, who declares, he "always differed in opinion with medical writers on the subject of the importance of the functions of the uterus." One who contemns the opinions of all his predecessors and contemporaries, should employ something more convincing than flippant declamation.

But does Mr. F. really differ from all medical writers on some of the functions of the uterus? Certainly he does not. We have shewn that Hippocrates, two thousand years ago, was of the opinion now entertained by Mr. F. on the nature of the menstrual discharge; and from his time down to the present moment, he has been echoed by hundreds. And we are not a little surprised that Mr. F. did not take advantage of their aid, instead of bidding defiance to the whole of them; for with some, authority is paramount to fact, and by its influence he might have obtained some credence.

Within a period of a few years, attention of no mean kind has been given to the nature of most of the secreted fluids of the human body; and, by the aid of chemical analysis, we are in possession of many valuable facts on this subject; but we have still to lament, that no well conducted experiments on the nature of the menstrual fluid have yet reached us; we are, therefore, to rely on the sensible and physical properties of this fluid to disprove its identity with the common circulating mass. But these



are so strikingly clear, as to induce us to rest our cause upon them, and to make us insist that the menstrual fluid is the product of the "complicated bodies called glands," and entitled, from this circumstance, to "more important considerations," agreeably to Mr. F's own confession.

"When the female is in health," says Mr. F. "the various secretions and this effusion go on ; but on the system being reduced by disease, the glandular secretions continue, though in smaller quantity ; this effusion stops, nor can it be restored till the system is replenished."

We would ask if this is a statement of facts, or mere phraseology in conformity with pre-conceived opinions to which facts must bend ? It is true, Mr. F. may shield himself by the vagueness of language ; such as "when the body is reduced by disease,"—for this may mean something or nothing ; "reduced" is a very equivocal expression. But if he mean by it the mere debility consequent upon an attack of an acute disease, we should without hesitation deny that "this effusion stops, nor can it be restored until the system is replenished." Every practitioner of common observation must have noticed, that, in fevers of every type and denomination, and where debility had existed in all its grades, that this "effusion" did not stop when the body was "reduced," nay very much "reduced." Friend,\* who, of all others, had more at stake to prove a superabundance of blood, admits, without hesitation, as true, the following objection to plethora being necessary to the production of the menstrual discharge, viz. that, "in tabid persons the menses appear according to custom, nay, and sometimes to excess ; in whom the quantity of blood is below a medium." This fact being notorious, he was obliged to acknowledge it, though he tortured his imagination to do away its force.

Indeed, so little is this discharge under the control of the general contingencies of the system, that we cannot affect it by any detraction of blood, however near the ac-

\* Emmenol, p. 35.

customed period it may be drawn. Nay, farther, not even long continued hæmorrhages will arrest this secretion in its usual march, as the following histories will prove: In the year 1787, my preceptor was called into consultation with the late Dr. Carson of this place, to a young woman who had been afflicted with a periodical discharge of blood from the ear for several months; it recurred every day about 11 o'clock, A. M. and would discharge three or four ounces of blood ere it would cease; during the whole of this period the girl menstruated regularly, and in the usual quantity, although much reduced by her disease and remedies. We have, at this moment under our care, a lady 28 years of age, who has had for the last five or six years of her life, an almost daily discharge of blood from the bowels; she will frequently void a pint or more of blood per diem, yet she menstruates with the most perfect regularity, and is never less than six days in this condition. This lady is, as might well be expected, feeble, pale, and wretched.

Plethora is so unimportant to the menstrual evacuation, that this very condition is, in many instances, perhaps chargeable with a suppression of it. This is conformable to the opinions of many authors on this subject,\* and has been many times confirmed by our own experience. And we may here advance, without fear of error, that the blood which is capable of maintaining the general system, however small that quantity may be, will be sufficient to enable the uterus to produce the catamenial flow, provided this viscus has no other evil to contend with than scarcity of blood. And that, when this flux is interrupted by chronic disease, as phthisis, obstructed liver, scrofula, &c. it is not owing to the want of blood, but to an interruption being given to the sympathies that excite this secretion.

"I have said," continues Mr. F. p. 176, "it is an accommodating organ. It can be extended till it can con-

\* Riverus, Ettmüller, &c. Friend's Emmenol, p. 38.

tain one, two, or more gallons for several months ; and, soon after being emptied, it contracts to its usual shape and size. It is on such occasions alone that it may be called a truly important organ ; on all other occasions, it is the least important of any organ of the same size in the system."

Agreeably to this, the uterus is only "an important organ" when in a state of distention. We presume Mr. F. means when that distention is occasioned by pregnancy. So that according to this statement, when it is in a state of vacuity, it is of no kind of use. But if it was not of "importance" before it became "distended," it could not have been "distended" to become of "importance ;" for before this condition takes place, there are several *important* functions to be established by and in this viscus—first, there must be the menstrual secretion, and this of a certain kind, for the mere circumstance of an evacuation of a coloured fluid from the uterus is not sufficient, as is evinced, where this evacuation is accompanied with coagula, or a deciduous membrane, or is too sparing, or too abundant, or where the period is anticipated, or protracted. Second, there must exist in this viscus the capacity to produce the decidua, that the ovum, when deposited within it, may be properly attached and developed : for without this, impregnation could be of no avail ; and instances have occurred within our own observation, where there was every reason to believe that the decidua was not secreted, or was improperly produced.\* We therefore are of opinion, that a healthy condition of the uterus is of primary importance to its being put into a state of "distension," or before it can become "an accommodating organ." Hæmorrhage from this part never insures a fortunate issue

\* We have seen several instances where ova were covered with an efflorescence of the spongy chorion ; these ova were thrown off at different periods, from six weeks to nearly three months. No discharge of blood accompanied these abortions, at least nothing more than what the women would term "a show ;" in these instances it was presumable the uterus had not furnished a proper membrane, and the chorion did not unite with it, in consequence of this imperfection. This has happened several times to the same female.

to impregnation,\* let it recur with ever so much regularity; of this we have abundant proof in all the cases where this prevails, as the woman for the time being is uniformly barren. This we advance without the fear of contradiction, as the fact is perfectly notorious. A discharge, then, "of real blood, unchanged," from the uterus, instead of the healthy product of menstrual secretion, is sure to entail an inability to procreation. Can Mr. F. be right, then, when he says, "that in all other occasions, except when in a state of distension, it is the least important of any organ of the same size in the system?"

We must also deny that the uterus† is that "passive organ" that Mr. F. so sneeringly insists on. Its direct powers, which we have but in part glanced at, and extensive influence on other parts of the system, through the medium of its varied and extensive sympathies, make it, in our opinion, instead of a "passive," one of the most active "organs" of the body; and, although we are not prepared to say strictly with Van Helmont, "*Propter solum uterum mulier est, id quod est*," yet we cannot shut our eyes against the evidence of every day's experience, that much, very much, depends upon the healthy condition of this viscus. Shall that be called a "passive organ," which has, if we may so term it, the care of the re-production of our species? And has it virtually less than this, when a derangement of one of its functions shall circumvent this great object of human concern? Did Mr. F. really ever know an instance, wherein the catamenial discharge was interrupted as an idiopathic disease, fail, if long continued, to involve the female in ill health? Did Mr. F. ever know an instance of impregnation where amenorrhœa existed? Should

\* While coagula are discharged from the uterus at the menstrual periods, the woman never becomes a mother; this may not, and perhaps does not, prevent an ovum being impregnated, yet this impregnation is never conducted to fortunate issue. The ovum may be deposited in the uterus, but it does not find that viscus in a proper condition to receive it.

† By the uterus we would mean the uterine system; ovaria, tubes, &c. as they cannot be considered but as forming a whole.



Mr. F. answer these questions in the negative, as we presume he must, he cannot say the uterus is a "passive organ," but must agree that the uterus, *in a state of health*, is absolutely essential *to a state of health*.

Mr. F. says, "when the functions of any organ are destroyed, the organ may be said to be destroyed; it then exists as no better than if removed from the animal. We have daily experience of the functions of the uterus being destroyed in the largest and the smaller domestic animals. So far from the animal being injured with regard to health, that it grows fatter, and thrives better, is happier without, than with these functions. The organ has been removed by the knife without occasioning the death of the animal. So much for its importance!"

We agree with Mr. F. that "when the functions of any organ are destroyed, the organ may be said to be destroyed;" but is this the subject in question? Certainly not. Mr. F. does not ask, "if we destroy the functions of the uterus, is that uterus, as regards the intention of its formation, of any further use?" No; he endeavours to prove, it is of no use while it possesses all its powers. But we contend, that upon the due and healthy performance of its functions, the health of the system at large very much depends. Besides, is the utility of the parts of the system at large to be determined absolutely by their loss? Will it be contended that an arm, a leg, an eye, a spleen, &c. are of no use, because the animal has not died, or even suffered the loss of health by their removal? Is this correct reasoning? Does not Mr. F. attempt to prove too much, thereby prove nothing, when he says, "we have daily experience of the functions of the uterus being destroyed," and, "so far from the animal being injured with regard to health, that it grows fatter, and thrives better, is happier without than with these functions?" Does he not attempt by this to prove the uterus is injurious to the animal, and indirectly declare, he could manage "these matters" better than an all wise God, who had the framing "of all things," and who did, with infinite wisdom, frame

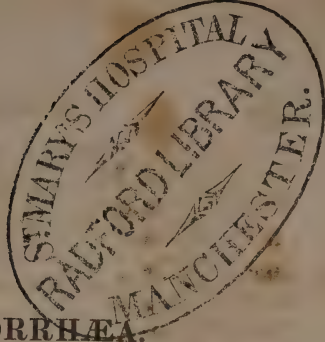
"all things?" Did Mr. F. ever read the fable of the philosopher and the acorn? An omnipotent and omniscient God knew less well how to make his creatures happy, than our author; for Mr. F. would not have had the *cruelty* to give uteri to females, as they would "*thrive better,*" and "*be happier,*" without them!

Pray, what proof has Mr. F. that an animal is happier after it has been deprived of its uterus? Has he ever been able satisfactorily to make the inquiry?

"I will place," continues Mr. F. "this subject in another point of view. The female enjoyed the best health and spirits from birth, till she arrived at the age called of puberty, suppose fifteen. At this period an effusion of a small quantity of blood took place from the organs of generation, and which returned at periodical times, till she arrived at another age, say forty-five; the effusion then ceases at once, and never returns. What was the consequence? Nothing at all. Did she fall into a bad state of health? No; she continued in good health for thirty years more, and died of old age. Instead of suffering any injury from the cessation of this effusion, she was happier and more comfortable without it. I now ask a question, I hope without any impropriety, as it is chiefly to gain information, of what use or importance was this effusion for the best thirty years of the female's life? I confess I cannot see any; I must be understood to mean with regard to health or disease."

The whole of this long quotation, we conceive, can be answered in a very few words. The uterus begins to perform its functions at the period of puberty—the healthy performance of these functions is essential to the great object for which this organ was designed—it performs these functions agreeably to the design of God, for a limited time, and they then cease agreeably to the same design; and in all this we perceive marks of infinite goodness; for it was not intended this secretion should continue beyond the time at which a woman could have a reasonable prospect of rearing and protecting her family. And its ceasing at

this period is the strongest proof of its utility during the period of its flow. And Mr. F. must be well aware he has taken an extreme case, when he conducts a woman from the moment of her birth, and makes her represent the whole sex, to the instant of her death, in very advanced age, without the individual having suffered some inconvenience, should there have been, during any period of that time, any morbid affection of the uterus; and if there was none, it would only prove that, when this part is in a state of health, the woman cannot suffer from this cause.



## ON DYSMENORRHÆA.

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It is a matter which justly challenges surprise, that this painful affection of the uterus should have attracted so little notice from the earlier writers on medicine ; and we cannot help feeling it a reproach, that almost the whole of the later medical authors are involved in the same charge of supineness towards a disease which merits a most serious consideration. Was there no other inducement to the study of Dysmenorrhæa than to relieve pain, we should hold it sufficient ; but there is a much higher motive connected with it ; for when relieved, the married woman who was by its continuance doomed to barrenness, becomes fruitful, and fulfils the duties of her creation. It was not until the time of Dr. Denman, that this complaint attracted particular notice ; and it is to him we are chiefly indebted for its history and real character.

Dr. Fothergill, it is true, has vaguely noticed it,\* and Dr. Cullen has given it en passant a place in his "First Lines," but neither has adverted to its most remarkable attendant, the discharge of a membrane. Dr. Denman appears to be the first to have considered this substance as constituting in part the disease ; and has given a short, but clear account of the escape of this substance from women who menstruate with difficulty. Morgagni,† however, long before him had given a very remarkable case of this kind, which has not been noticed by the Doctor ; it must have escaped his notice or his recollection, or he would

\* Works, p. 468.

† Epist. xeviii. Art. 12.



not have failed to cite it, as it is one of the most extraordinary instances of this kind upon record.

But, although Dr. Denman has noticed the formation and escape of this deciduous substance, yet he has not attempted to give an explanation of its formation, or the consequences to which it gives rise. He merely states that "the pain is to be attributed to an increased degree of irritability in the habit, or to the difficulty with which those vessels designed for the menstruous discharge, became permeable," which in fact, is saying nothing: for a mere increase of irritability does not necessarily imply an increase of sensibility; and "the difficulty with which those vessels designed for the menstruous discharge to become permeable," gives, in our opinion, no adequate idea of the cause of the pain which attends this disordered discharge, nor is it the cause of it.

Nor can we regard Dr. Fothergill's explanation as superior, although Dr. Denman thought it sufficiently important to almost copy it. Dr. F. says, "this excruciating pain seems to be spasmodic, and to proceed from the extreme irritability of the uterine system: The blood naturally determined hither, in order to its being discharged, by distending the very irritable vessels, occasions the spasms; this produces a constriction of the vessels; they became impervious, and the nîsus to the discharge continuing, the pain becomes exquisite and general, till the patient, worn out with the struggle, is debilitated and sunk; the fluids are then dismissed, some ease succeeds, but the patient is often so reduced as not to recover her usual strength before she has another conflict to undergo."\*

Dr. Fothergill has however remarked one striking feature in this complaint, namely, its disqualifying the woman from becoming a mother. He observes, "it would seem as if the uterus itself was so far a sufferer as to be rendered by degrees less fit for fecundation. I think it has been observed by other Physicians, as well as myself, that

\* Works, p. 468.

few of those who have suffered much in this manner here described, have borne children.”\*

We shall endeavour, then, to give an explanation of this distressing condition of the uterine system upon very different principles. In prosecuting our inquiry, we shall first notice the history of this complaint; second, attempt an explanation of the formation of the deciduous membrane; third account for the pain which invariably attends its discharge.

This complaint for the most part commences in women who are obnoxious to it, with the first menstrual periods, and unless prevented, most pertinaciously continues at subsequent return of the catamenia.† We have never observed any particular constitution or temperament especially liable to it. We have witnessed it both in the delicate and robust; in the sanguineous and the phlegmatic. The discharge commences sparingly for some time, and is then for a short period, almost altogether arrested; so soon as this happens, pain is felt, and this returns and intermits like the pains of labour—after a continuance of these alternate pains for an uncertain period, relief is sometimes suddenly experienced, and there is found discharged from the vagina a membranous substance of uncertain size,—sometimes it resembles, when spread out, the form of the uterus; at other times it is broken into fragments, but always maintains its membranous texture.‡ So soon as this membrane is completely thrown off, the woman is relieved, unless there be a fresh production of this substance to stimulate the uterus to new exertions, and to new torments; this is by no means unusual, and several days are sometimes employed before these efforts cease—at other times a few hours are all that is required to restore

\* Works, p. 469.

† The woman may however become subject to this complaint at any period almost of the menstruating time of life. I have in more instances than one known it to follow abortion.

‡ Dr. Denman declares this membrane to be smooth on one side and flocculent on the other; and this observation is confirmed by my friend Dr. Horner, who kindly examined a portion of it for me.

the woman to tranquillity. It is remarkable, that the quantum of pain is not always in proportion to the quantity of membrane discharged—we have seen extreme torture from a very small portion, and less pain where the deciduous product was considerable. But this is not difficult to account for.

The virgin and the married woman are equally the subjects of this distressing complaint. We have known it to commence immediately after marriage where it had not previously existed; and on the other hand, we knew it once to cease after this consummation.

Beside the alternate pains which we have just noticed, there is almost always a distressing aching in the back and hips, and which almost invariably announces the approach of the period; nor does this cease, in many instances, until two or three days have elapsed after the catamenial flow.

Having thus rapidly given the history of this complaint, we will endeavour to account for the formation of the membrane.

It seems now to be a fact generally admitted, that the menstruous fluid is the product of a secretory process. I taught this doctrine in my first course of Lectures on Midwifery in the winter of 1796—7, and at the time believed it was original with me; but upon mentioning the theory to my friend Dr. Physick, he informed me that it was taught in London by Dr. Clark, who gave the credit of it to Mr. John Hunter. I was lately informed it was first suggested by the celebrated Bordeu; but from the hasty glance I gave his chapters on the glands and menstruation, I could not discover it—it may however be in his works.

Be this as it may, we have strong evidence that the blood, after it is thrown out of the uterine vessels, is very much changed from the common mass of this fluid. It differs from it in the following particulars: 1st. It is much thicker. 2d. It does not resemble it in smell. 3d. It is much darker coloured. 4th. It never separates into

its constituent parts. 5th. It never coagulates. 6th. It is said to be nothing like so susceptible of the putrefactive process.

From this it would appear, that the coagulating lymph receives a new modification during its transmission from its vessels to the internal cavity of the uterus, since it no longer exhibits its common property of coagulation when exposed; and this circumstance becomes a test, that the uterus is performing its menstrual duty properly; and on the contrary, when this does not obtain, it marks a diseased condition of this viscus.

It may be asked, why this change in the coagulating lymph should be necessary? We answer, it is a wise and kind provision; and that, the peculiar process by which it is formed, is, in our opinion, chiefly directed to this end. And to effect this, nothing more is required than a specific arterial action; for this kind of change takes place in many instances in the general circulating mass from some peculiar impression on the sanguiferous, or perhaps nervous system—thus in yellow fever, scurvy, death from a blow on the stomach, or electricity, passions of the mind, inordinate exercise, &c. we have dissolved blood as it is termed, or blood which does not coagulate.

For the purpose of operating a change on the coagulating lymph, the process of secretion is instituted, or at least the blood designed for the menstruous discharge is subjected to the action of certain vessels, whose office is to deprive it of the power of coagulation, and in the healthy state of the uterus, this act is faithfully performed. And we believe that this is the principal change that the blood undergoes, and this for the benevolent purpose of exempting the female from the long continued pain and suffering that would necessarily ensue, did not the uterus perform this kind and friendly office. Did the blood retain the property of coagulation, the life of the woman would truly be a life of misery, as at every menstrual period she would suffer the miseries of a labour; the blood would coagulate within



the uterus, and this viscus would be urged to contractions of the most painful kind to throw it off; and no sooner would it have achieved this desirable end, than a fresh and similar duty would be imposed upon it, and would thus continue until the period should have passed over. And in cases of imperforate hymen, what would not the poor woman be doomed to suffer from the same cause? But here, a kind Providence interposes, and by the arrangement of the menstruous blood remaining fluid she escapes from the misery that its coagulation would produce.

Since then we can show a direct advantage in the menstruous blood remaining fluid, is it not more than mere conjecture to say, that as this change was an important one, that the process of secretion was instituted with the express intention to impose this alteration on the coagulating lymph? We are ourselves decidedly of this opinion. An opinion which, however at variance with many respectable authorities, is well supported by facts, and the phenomena of menstruation. Did no advantage result from the change we have been contending for, it might be idle or unimportant to insist on it; but, as it is a remarkable circumstance in the history of the menstrual discharge, that, *in a healthy state it never coagulates*, we have thought proper to insist on this peculiarity as evidence, of a change of a notable kind. And that this change can only be the result of a secretory process.

It has been said\* that the menstruous blood is prevented from coagulation by its admixture with the mucus it may meet with in the vagina. But this is purely conjecture. It never has been proved by direct experiment; nor is the mean, in our opinion, sufficient for the end. In the case of imperforate hymen the menstruous discharge remains fluid; this perhaps has been collecting many months; and as it is entirely confined to the vagina and cavity of the uterus from the first period of its secretion until it may be discharged by art, and without being subjected to any con-

\* Mauriceau, Haller, &c.

quasitory motion to incorporate it with the mucus secretion of these parts, we cannot see how mere contact with it, should so change the lymph as to prevent its coagulation.

Having (we trust) rendered it more than probable, that the fluid thrown out at the menstrual period is the product of a secretory process ; and that this process is instituted with the view to deprive the coagulable lymph of the power of coagulation ; and that when this secretion is healthily performed this end is uniformly effected ; let us advert to the consequences that would follow, supposing that from some cause or other, an interruption is given to this healthy condition of the uterus : it would seem, under such circumstances, to follow as a consequence, that the fluid discharged would differ from the product of a healthy and well established secretion. The process would be imperfectly performed, and the required changes would not be completely induced,\* the coagulating lymph would not be entirely deprived of its usual or common capacity, consequently the menstruous fluid would be imperfectly elaborated ; so soon then as this fluid is eliminated from the secretory vessels it will begin to separate into its constituent parts, the colouring matter will separate from the

\* It may be questioned by some, (if the menstruous fluid be a secretion,) whether there is any coagulating lymph in this discharge, since it does not manifest itself by this power, and consequently if it be the case that there is no coagulating lymph in the menstruous fluid the explanation we offer must necessarily fall to the ground ; to a supposition of this kind we would answer, that the presence of the lymph is rendered more than probable ; first, from, in every instance where the experiment has been tried, where the red globules of the blood were found, the coagulating lymph has been proved to accompany them. 2d. That, as there is in Dysmenorrhæa almost invariably a casting off of a membrane, it proves the existence of the coagulating lymph, since we know of no other fluid of the human body capable of producing a membrane-like substance. Besides, were it ever to be proved that in the healthy secretion of this fluid there is no coagulating lymph, still it would not disprove the explanation we offer of the membrane, as Dysmenorrhæa is occasioned by a diseased or vitiated state of the secerning surface of the uterus, consequently, this part may perform its function so imperfectly as to allow of the escape of the coagulating lymph, and this may, and most probably does, exert its unsubdued powers so soon as it escapes from the uterine vessels.

imperfectly subdued coagulating lymph, and will, from its superior density, occupy the lower or most depending part of the uterine cavity and will sooner or later make its escape, while the coagulating lymph will remain either altogether or in part to spread itself over the internal face of the womb, and will, as it is wont to do when in contact with living parts, quickly assume the appearance and density of membrane.

This membrane will be to all intents and purposes an extraneous substance to the uterus, and will consequently stimulate it to the effort of throwing it off, which will be eventually effected by the institution of alternate contractions ; and *hence the pain* during this process.



AN ESSAY ON THE

## RUPTURE OF THE UTERUS.

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THERE is great reason to believe that the uterus is ruptured during parturition much more frequently than is commonly supposed. Many of the sudden deaths during labour or quickly after, are entirely unaccounted for, though the symptoms attending the illness justifies the belief, that they were from this cause. At this we need not be surprised, when we reflect how few are properly qualified to practice midwifery; many male, as well as female practitioners, being entirely ignorant of the first and most common principles of the art, and consequently wholly incompetent to appreciate the danger, or determine the nature of the accidents which may accompany, or speedily follow the process of parturition.

But cases of this kind are not concealed by ignorance alone. Not a few have been hidden, lest the disclosure might injure reputation. This want of candor has been perpetuated by two causes; the first proceeding from the express recommendation of the otherwise liberal Dr. Smellie; and the second, by the unsettled state of the question, "what, or if any thing, should be done for the relief of the patient, when this melancholy accident has taken place?"

Dr. Smellie\* says, that in a case he was called to, "on introducing the hand to deliver the placenta, the uterus was found torn at the fundus, and the intestines pushed down."

\* Vol. iii. p. 386.



“In order to avoid reflections, this accident was kept secret.” And, in p. 386. a correspondent tells him in a similar case, that “according to his *prudent* advice, he spoke nothing of the matter.” Here then is high authority for the concealment; and it is to be feared that too many have availed themselves of it.

But, the unsettled practical point has led to more numerous concealments than the first—for while the practitioner is balancing in his mind the propriety or impropriety of acting, the patient expires; and then the fear of injury to his reputation, should he candidly make known the state of the case, forever seals his lips. To settle then this agitated question, is the principal object of this paper. We pretend to but little novelty in this discussion; our chief aim is to remove, as far as may be in our power, all ambiguity from the subject, by reducing our duty to a few, but important rules.

In prosecuting this object, we will first inquire whether it is proper to attempt any thing for the relief of the unfortunate woman who may have suffered this accident? Secondly, enumerate the causes of it, with their mode of action. Thirdly, detail the symptoms and consequences of the rupture. Fourthly, point out the mode of proceeding, under all the various circumstances with which this accident may be complicated.

1. The late Dr. Hunter considered it an act of wanton cruelty, to disturb a patient who had suffered this accident, by attempts to deliver her—so inevitable did he consider her death; and so imposing was his authority among the British and American practitioners, that it became an undisputed point of doctrine, and to have departed from it, would have been considered a species of murder. This opinion was founded in error, and perpetuated by the force of education; and its propriety was not seriously called in question in Great Britain, until about the year 1784; at this time a case fell under the notice of Dr. Douglass,\* who

\* Essay on Rupture of the Uterus, p. 7.

*dared* to differ from the established authority, by delivering his patient, and was rewarded for his good sense and zeal, by her recovery. Examples of this kind had occurred on the continent of Europe, but they had failed to make the impression they deserved. The medical public are much indebted to Dr. Douglass for having ventured to dissent from the almost universal opinion of his fellow practitioners; and he has taught them by his intrepidity, what they failed to learn from the experience of the continental accoucheurs, that the death of a woman who had suffered a laceration of the uterus, is not inevitable. The case just noticed made for a time a strong impression; but the infrequency of the accident, and the force of pre-conceived opinions, but too soon effaced it.

When a case occurred, authority was sought for in the writings and opinions of men whose influence and credit was greatest with the medical public; and they were found but too often consonant with those promulgated by Dr. Hunter and some other teachers of high character: hence we find, that "not to give pain where we could not do good, became an aphorism," in the treatment of the rupture of the uterus.

It is truly a matter of surprise from whence this opinion originated, as it had neither reason nor fact to support it—it was taken for granted, what remained to be proved, or perhaps more properly, was manifesting a total disregard of facts, as incontestable in their nature, as important in their consequences. Had Dr. Hunter, and others who espoused his notions, never have heard of the recovery of the woman, where delivery had been resorted to, we should be less surprised; but as we cannot suppose them ignorant of what had happened in other countries, we are totally at a loss to account for their opinions on this subject.

We cannot withhold our astonishment, when we receive from Dr. Denman\* the following declaration; and the more especially, as his extensive practice and reading should

\* Essay on the Rupture of the Uterus, p. 17 and 18.

have led him to contrary conclusions. "If the principle on which all operations in surgery are justifiably performed, be to lessen or to remove present danger or suffering, or to prevent greater, though distant mischief; it may be reasonably asked, what benefit can result to the patient or to society, or what credit to the profession, from an operation by which her present sufferings are in some degree aggravated, and by which neither the lives of the parent or child were ever known to be preserved, with whatever knowledge or dexterity such operation may have been performed? If we further take into the account, that the few instances of recovery or escape from danger, when this deplorable accident has happened, (if there be no error in the accounts transmitted to us) were those in which no operation was performed, we may be warranted, without presumption, in coming to the following conclusion; that,

*"When the uterus is ruptured at the time of labour, both reason and experience show, that the patient has a better chance of recovering, by resigning the case to the natural efforts of the constitution, than by any operation or interposition of art."*

"If those deductions be legal and this conclusion just, in cases of the rupture of the uterus, it will follow, *a fortiori*, that in lacerations of the vagina, the patient ought not, for that reason, to be delivered by art."

We have already expressed our astonishment at these declarations of Dr. Denman, because they are in defiance of both "reason and experience," and directly contradicted by the testimony of the most respectable authorities. And the premises and conclusions are the more faulty, as no proofs are offered of their validity. We shall therefore prove, that there are instances of recovery from this deplorable accident, where there was an "interposition of art," by relating cases from authors of deserved faith, and challenge the advocates of Dr. Denman's opinions to prove, that there ever was an instance of recovery, "where no operation was performed."

The first instance that is distinctly recorded, is that men-

tioned by Heister,\* on the authority of a surgeon named Rungius. In this case, the intestines were distinctly felt through the rupture of the uterus, and through which the fœtus was extracted ; yet the woman recovered. Dr. Douglass, in his Essay,† gives the history of Mrs. Manning, who also recovered. Dr. Hamilton‡ relates another instance of complete and entire recovery, although the intestines issued through the wound of the uterus, and were reduced by him after the delivery of the child. In this case he declares, “ the recovery was nearly as good as if no extraordinary accident had happened.” Dr. Ross relates the case of a Mrs. Granan of Eppendorf, near Hamburgh, who suffered this accident in two consecutive labours, and yet recovered.§ Mr. Kite gives a case of ruptured uterus, which terminated favourably.||

In a copy of the MS. lectures of Dr. J. Hamilton, the present Professor of Midwifery in Edinburgh, there are two cases related of recovery, one of which he himself attended, and says, it was “ one in which almost every circumstance was unfavourable ;” for “ in bringing the child through the lacerated part,” he “ felt the uterus tearing more ; the woman lost three pounds of blood, yet she recovered, and afterwards had children.” The other case occurred in Lancashire ; “ a poor woman fell from a cart, in consequence of which, the uterus was ruptured, and the child passed into the abdomen ; the bones of the pelvis were so much injured by the fall as not to allow of delivery, being much *mashed*, the Cæsarian operation was performed, and she recovered.”

Mr. Thibault¶ relates a similar case to the one just recited—gastrotomy was performed with the most entire

\* Instit. de Chir. tom. ii. p. 137.

† Essay on the Rupture of the Uterus, p. 7.

‡ Outlines, p. 344.

§ Annals of Medicine, vol. iii. p. 377.

|| Mem. Med. Soc. of Lond. vol. iv. p. 253. Madam La Chapple also ; Annuaire Medico-Chirurg. tom. i. p. 542.

¶ Jour. de Méd. for 1768.



success to the woman, though too late for the preservation of the child.

Baudelocque relates\* that a M. Lambron, a surgeon of Orleans, performed gastrotomy twice on the same person, with the desired success to the woman, after the rupture of the uterus. This woman became pregnant a third time, and was delivered naturally of a healthy child.

Mr. Hugo† relates a successful case also, and we could without difficulty increase the number; but these are sufficient to prove that success has attended the "interposition of art." The cases we have just cited, were all fortunate to the woman, but the child uniformly perished; this was rather owing to the time at which art interfered, than to the mode it adopted. Of this we have sufficient proof in the case related by Burton.‡ He says "I was called to the wife of a broker in the city of York, who had had several children—she fell into labour at the regular time; she had only a slow labour at first, but after some little respite, her pains became more violent, during one of which she perceived something to crack within her, as she termed it; after which she exchanged her pains for faintings, &c. with an intermitting pulse; on this account I was called in: being told every thing that had happened, I was apprehensive of what, indeed, proved to be the case; wherefore, I told the by-standers my opinion; and that, as the child was alive, it was proper the woman should be delivered as soon as possible, which was done directly. The child was small, but very healthful and lively. Immediately after the birth, I introduced my hand into the uterus, where I found one side of it burst so wide, as to have admitted my hand to pass through the opening." Mr. Haden relates a case that terminated with safety to both mother and child.§

We have said we would challenge the advocates of Dr. Denman's opinion, to furnish a single instance of recovery, from a rupture of the uterus or vagina, at full time, when

\* Heath's Translations, vol. iii. p. 430.

† Med. and Phys. Jour. for March, 1803.

‡ System of Mid. § 43, p. 110.

§ Med. and Chirurg. Trans. vol. ii. p. 118.

the fœtus had been permitted to remain undelivered ; and we do this without the fear of its being accepted. We are well aware, that histories purporting to be of this kind, are before the public ; but there is strong reason to call their identity in question. We believe these to have been cases of extra uterine fœtuses, where recovery has been said to follow ; we believe this, because, in cases of this kind, at the end of nine months, pains resembling those of labour have come on, and continued for a certain period—they would then disappear, not again, perhaps, to return. But in no one instance in cases of this kind that we have yet examined, have the same symptoms which mark the rupture of the uterus occurred ; and it would be easy to cite those where death has eventually ensued, in which it is expressly stated the uterus was found sound. Now, would this be the case, had the uterus suffered a laceration ? would not the accident quickly have declared itself ? and should we not have found traces on the uterus when examined after death ?

We are the more firmly persuaded of the truth of our position, by a case of this kind which recently occurred in this city. In this case, at the usual termination of pregnancy, pains were experienced, but the patient was proclaimed by her accoucheur, to be neither in labour nor pregnant—fifteen years after this she died, in consequence of an abdominal abscess—upon opening the body, a fully developed fœtus was found enclosed in a membranous sac—the uterus perfectly sound, and without blemish.\* I was favoured with a sight of this fœtus and uterus, through the politeness of Dr. Harlan, and refrain from saying more on the subject, as I hope to see a detailed account from the gentleman who attended her.

This case we feel to be strictly in point ; first, because pains like those of labour were experienced, which, after a period ceased, and which might be considered by those who espouse the contrary opinion to ours, as a case termi-

\* See also Dr. M'Knight's case, *Mem. Med. Soc. Lond.* vol. iv. p. 342, &c. &c.

nating by the rupture of the uterus, and from which the patient might be said to have recovered. Secondly. Because dissections clearly prove that these symptoms may occur without the uterus being immediately interested, as it has been found entirely sound. Thirdly. Because we are of opinion that this case completely explains the supposed recoveries from rupture, where the child was permitted to remain within the abdomen.

We are aware that there is a case recorded in the *Journal de Medecine*, as a case of ruptured uterus,\* from which the woman recovered, though the fœtus was suffered to remain in the abdomen. But this case is not strictly in point, and is to be admitted with great suspicion, as an instance of a child escaping into the cavity of the abdomen, after a lesion of the uterus. First. Because the woman was but four months advanced in her pregnancy; and consequently her case does not infringe upon our general position. Secondly. Because, as it is recorded as a case of ruptured uterus, it is more than probable that it had some agency in the woman's death four months after the accident, although not thus stated; the body was opened to ascertain the cause of death; as the woman menstruated regularly after the accident which caused the rupture, great doubt was entertained of her pregnancy. Thirdly. There is strong ground of belief that the fœtus did not entirely escape into the cavity of the abdomen, although it is expressly stated that "a fœtus was found in its cavity;" but this may mean no more than, that upon opening the abdomen, a fœtus was discovered within its parietes, without intending to declare it was not enveloped in a membrane or sac, and which membrane or sac was the peritonæal coat of the uterus,† through

\* *Jour. de Med.* for 1780.\*

† We have, in support of this opinion, Dr. Ross's third case, *Annals of Medicine*, vol. iii. p. 306. "On opening the abdomen of this cadavre," says Dr. Ross, "it was found under the ligamentum latum of that side, (the left) an arm of the child could be felt, covered only by the peritonæum." Here then is demonstration that the substance of the uterus can be torn without doing violence to its peritonæal covering.

which it had not passed. It is not stated that it was found lying loose among the intestines, which we should expect to have been declared, had it been the fact; but merely that it was found within the abdominal cavity. Now we believe this to be strictly true, but does this prove it was not arrested by the peritonæum? We sincerely believe it was.

First. Because we can readily imagine that the substance of the uterus may yield, without disturbing the integrity of its peritonæal coat, and the more especially at this early period of gestation, as it is loosely spread upon it, that it may not suffer undue distension in the advanced periods. Secondly. This supposition is strengthened by the appearance of the uterus itself; for it is said that "a rupture was discoverable in the uterus, which was closed and cicatrised on the internal surface, but still open in that towards the abdomen." Now we believe that the wound was prevented from closing towards the belly by the presence of the fœtus, which being in contact with it, acted like an extraneous body, and thus prevented its healing.

These suggestions, we think, are corroborated by the symptoms which immediately followed the accident, namely, "an uterine hæmorrhage." This discharge proceeded most probably, from the lesion of the uterus itself—after a time "the flooding ceased, and the menses appeared;" that is, the discharge of blood from the wounded uterus ceased, so soon as the wound was "cicatrised on the internal surface," and thus restored to its former integrity, "as the menses" (an evidence of its healthy condition within) "appeared at the end of a month."

No mention is made of coagula of blood within the cavity of the abdomen, nor of any other extravasated fluid; it is probable then that none existed. But would this state of things have obtained, had the rent passed through both the uterus and the peritonæum? We think not—for, as there was an external discharge, it is more than likely there would have been internal extravasation. But to put this matter almost beyond question, we will relate two cases



where there can scarcely be a doubt that the uterus may be so ruptured, as not to produce the solution of continuity of its peritonæal covering ; the first is thus related by Dr. Bell.\* “ Elenor Noon, aged twenty-one years, after being married fifteen months, had the usual signs of pregnancy. At the expiration of her reckoning, was seized with labour pains, which by her own account, were very violent for several days, and continued near three weeks, gradually becoming more languid, until they entirely ceased ; after this, she perceived no motion in the abdomen. But as her pains abated, her belly increased in size, and continued to do so until I saw her,” “ about five months after she was seized with labour pains. At this time the whole abdomen was so much distended, that I imagined she and her friends had mistaken the case, and that it had been ascites from the beginning.” “ She was emaciated to the greatest degree, and so weak as not to be able to stand.” “ I judged it impossible that she could outlive a few weeks.” “ But upon going to the same part of the country about four months after, I found her still living. I was then informed that, about a month after I last saw her, all the water had been discharged from a small rent at the navel, and had come away in such quantities, and such violence, that near four gallons were received in a short time. Mixed with this water, were some fleshy strings, and as the oozing continued, some small bones came away.” In consultation, it was thought proper to dilate the opening ; this was accordingly done to the extent of four inches, and from the wound were extracted the bones of two fœtuses. “ My patient bore the operation better than could have been expected. No hæmorrhage ensued, and she recovered her health so very speedily, as to be able to menstruate in little more than three months. “ She became pregnant two months after, went her full time, and had an easy and natural labour, and a healthful child. Since that she has had six more, and enjoys as great a share of health and strength of constitution as most people.”

\* Med. Comm. Amer. edit. vol. i. p. 303.

“During the operation I introduced my hand several times into the cavity of the abdomen, both that I might extricate all the bones and other extraneous matters, and that I might be able to judge whether the fœtuses were in the uterus, or any cyst formed for them.” “Most of the gentlemen were of the former opinion; indeed, the thickness and firmness of the substance in which the fœtuses were contained, seemed to corroborate that opinion. But besides this, the woman still persists in asserting, that the waters came from her in that labour in the natural way, and in the same manner that they have done in several other labours since. This assertion, if true, must put it beyond all doubt that the fœtuses were lodged in the uterus, and not in any particular cyst.” We do not, by any means, agree with Dr. Bell in his last conclusion, as we believe there never has been an instance of a fœtus being retained within the proper cavity of the uterus, beyond a short period after the evacuation of the waters. We, therefore, think it was not so in this case: first, because a tedious and elaborate process was instituted for the removal of the fœtuses, when a much more simple and natural one would have succeeded better, namely, a renewal of the contractions of the uterus. Secondly. That the ulcerative process which nature established, could only have answered the end, by the destruction of a large portion of the substance of the uterus itself. Thirdly. Because we are expressly told she menstruated at “the end of three months,” and that “she had a natural and easy labour,” and became the mother of six more children. Now is it rational to suppose these things could have happened, had the uterus suffered so severely as it must have done, if the conclusion of Dr. Bell be admitted? We are of opinion this case admits of a much more simple explanation—namely, that the substance of the uterus alone suffered the lesion, while the fœtuses pushed its peritonæal covering before them, and were retained by it, and prevented from escaping into the cavity of the abdomen—the uterus healed after a time, and a sac was formed by the perito-

næum, in which the fœtuses were lodged ; and this became "thick" from inflammation, and the adhesions consequent upon it.

The other case we intend to relate is by Dr. John Sims,\* a physician of London of great respectability. "A well formed woman, mother of several children, seven months gone with child, after taking a very long walk, under great agitation of mind, was, upon her return home, seized with an uterine hæmorrhage which continued some days, and then gradually abated, and did not afterwards return ; but the woman continued very weak and ailing for two months, when, according to her reckoning, she had completed the full period of gestation. On the 11th of May, 1792, she was taken with labour pains, and sent for her midwife, who gave her expectations of a speedy delivery. But the pains going off, she left her in the evening, with assurances that the child presented right, and that every thing was in a safe way. Next morning, finding herself very ill, but without labour pains, the patient sent for an experienced practitioner in the neighbourhood, who attended, and upon examination, found the mouth of the womb not sufficiently dilated to admit the finger ; he could feel no membranes distended with water, nor any part of a child, either through the mouth of the womb or through the parietes of the womb itself. Her face was bloated, her legs and thighs œdematous, and her belly very large. From these circumstances, he very rationally suspected that she was not with child, and directed his attention to the hydropic symptoms. But as she grew daily worse, I was desired to see her on the 16th of May, when I found her unable to lie down in bed, complaining of violent pains in her side ; her respiration was short and frequent ; her pulse extremely rapid, with some hardness in the stroke ; a fœtid black discharge flowed from the vagina ; her legs and thighs were much swollen, and pitted upon pressure ; the mouth of the womb was relaxed and a little open at the first entrance, just as it is frequently

\* Med. Facts, vol. viii. p. 150.

found when unimpregnated in women who have had several children; no part of a child could be felt through the parietes of the womb, nor could the enlargement of this organ be perceived. I was immediately convinced that, if the midwife had given a true account of the case at the time she was first called, a rupture of the uterus had taken place, and the child had escaped into the cavity of the abdomen. With this idea, I examined the state of the belly externally, which was very large and hard to the feel; the tumour circumscribed as in pregnancy, but nothing like the extremities of a child could be felt through the teguments, and when afterwards the woman was able to lie down and turn in her bed, I could not find, upon examining in different positions, *that the tumour fell to the depending side.*

“ Upon the 18th she was better—the discharge from the vagina was increased and very fœtid—pulse one hundred and twenty strokes in a minute—she took tonics with advantage—in a few days more, some of the nails and a little of the hair of a full grown fœtus (as was judged by the size of the nails) were discharged from the vagina, and these followed by some small finger bones. She continued pretty much in the same state, till about the middle of June. By the first of July she was much recovered—the size of her belly was much diminished—the appetite good—and the discharge had entirely ceased. In short, the poor woman seemed now to be in a state of convalescence, when unfortunately she was persuaded by some foolish adviser, that a good jumbling in a coach would bring on her long expected labour—a coach was procured, and although the motion of it gave her excruciating pain, particularly about the navel, yet fully persuaded that it was from the shaking that she was to expect a salutary effect, she bore it with fortitude. The pain continued increasing after her return home, with great soreness over the whole abdomen, and she expired early in the morning on the 7th July, two days after the fatal ride.”

“ When the crucial incision was made through the inte-



guments of the abdomen, the lower flaps could not be turned down, on account of the adhesion of the peritonæal lining with the contiguous parts. But upon carefully dissecting them away, there was immediately discovered nearly the whole of the bones of a fœtus, which appeared to be of the ordinary size of a child born at the full time, or perhaps somewhat smaller." "This mass of bones was surrounded by a *membranous sac* of a black colour, which was entirely smooth on its internal surface, and adhered externally to all the contiguous parts. By these means a complete cavity was formed to contain the bones, *which had no communication with the abdomen.*" "In the anterior part of the cervix uteri next the bladder, was a rent *through its substance*, about three quarters of an inch in length, the sides of which were nearly contiguous, but ulcerated and not disposed to heal. Several small bones of the fingers and toes had fallen down from the mass above into the most depending part of the cavity, and were found lying about this opening. The uterus, very little larger than it is usually found in its unimpregnated state, was in its natural situation in the pelvis, and the fallopian tubes and ovaries were without any morbid appearance. The vagina was perfectly entire, so that there was no passage for the dissolved putrid matter of the fœtus, and the small bones that were discharged, but through this rent in the uterus, and thence through the mouth of the womb into the vagina."

This case we think perfect in all its parts, to prove that the substance of the uterus may be lacerated independently of its peritonæal covering; for it is expressly stated, that the rent was through its substance, and that there was *no communication with the abdominal cavity*, circumstances which could not have obtained had the peritonæum been ruptured, with the substance of the uterus itself.

But Dr. Sims draws a very different conclusion, and gives this case as one which "*shows the possibility of an escape, even when the child has been suffered to remain in the cavity of the abdomen.*" Does it prove this, either by

the detail of the facts or the event? Certainly not; on the contrary, it appears to us completely to support our position, that in all the cases recorded of recoveries from ruptured uteri, there was no direct communication with the abdominal cavity through the medium of the laceration, and consequently, cannot militate against our opinion, that there never has been an instance of success when the uterus has been ruptured at or even near the full period of gestation, and where the fœtus had escaped into the belly of the mother, and been suffered to remain there.

Dr. Sims calls this a case, where the fœtus had escaped into the cavity of the abdomen, though he expressly tells us that the sac containing its bones, "had no communication with the abdomen"—how then can the fœtus be said to have been in that cavity? That the sac containing it had one of its faces looking into the abdomen, we are willing to admit; but this does not place the fœtus there—there was an interposing substance between it and the cavity agreeably to his own statement, and that is all there is, when the child is contained in the uterus; and when there, it might as fairly be said to be within the cavity of the abdomen; or with equal propriety be said, that the lungs occupy a part of it, because only the diaphragm is between them. Would Dr. Sims declare the urine to be within the cavity of the abdomen, because the bladder occupies a portion of it?

Dr. Sims seems to be aware there was a difficulty in his way respecting this sac, and asks, "Was this sac entirely adventitious, and formed by a process of nature; or could it consist of the membranes naturally investing the child, retaining, by means of adhesion to the contiguous parts, enough of the principle of life to enable them to withstand the putrefactive process. The uniform black appearance of it may seem to countenance the last opinion; and, although it seems most probable that the membranes would give way at the part whence they meet with the least support, that is at the rupture, yet when we consider the proportion that three quarters of an inch bear to the whole

length of the uterus in its contracted state, it is not improbable that the rent might at first exceed six inches, and through so large an opening, it is easy to conceive, that the membranes might remain entire before the child, and tear off at the edge of the placenta, as we see often happens in quick labours; in which case the child might remain partly invested with its membranes. It seems even possible that the whole ovum may have escaped entire into the abdomen."

Dr. Sims seems inclined to believe that the sac in question consisted of the membranes proper to the fœtus, and appears at once to abandon the notion of its being of "adventitious" origin. Let us examine him on his own ground. First, he imagines that the membranes may be torn from the edge of the placenta, and by means of adhesion, retain sufficient life to resist the "putrefactive process," and thus form the sac. Now it is more than probable that the fœtus, &c. must have been protruded at the moment of the rupture, and that the membranes must have been torn from the "edges of the placenta" at the same instant; this being admitted, is it reasonable to suppose those membranes should retain sufficient of the "principle of life," to form adhesions after they had been forcibly separated from the part that supported that life? To effect this union would occupy some time—for inflammation must first be induced, and coagulating lymph must be poured out before this could take place—and though we cannot say with certainty what that period may be, yet we do not think we stretch the point, when we say it would at least require from two to three days—now we again ask, can it be supposed they would retain sufficient life to take advantage of this means of support, after having been separated from the source from which they derived their nourishment; especially as we know that when they are thrown off from the uterus after labour they quickly become putrid in hot weather, though the temperature of that weather is below the heat of the human body? Dr. Sims, indeed, seems conscious of this objection, and attempts to obviate it by saying they

may by means of adhesion retain "enough of the principle of life," to withstand "the putrefactive process." Is this not confessing that they would not "retain enough," did these adhesions not take place?—It follows then, that for the "membranes" to be capable of forming the sac, they must immediately have a connexion with the surrounding parts, or they will die. Now is it supposable, that at the moment the "rent" in the uterus took place, there was a provision existing to form this connexion? This would look like having the adhesive inflammation in readiness, and thus anticipate the necessities and contingencies of the accident. From what we know of the "membranes," we are not induced to believe they would form an union with any part of the body after a separation from the uterus, however important that union might be; for, even when partially separated from this viscus, they have never been known to unite again to it. In cases of uterine hæmorrhage, the spot from whence the blood was derived, is constantly indicated by a thin black coagulum, an indubitable evidence there was no re-union.

With respect to his last conjecture, "it seems even possible, that the whole ovum may have escaped entire into the abdomen," it is entirely destroyed by the statement of the dissection; for in this case, the peritonæum must be lacerated, yet we are told there was no connexion between the sac and the abdomen; nor any falling of the tumour when the patient changed from side to side.

Let us now endeavour to strengthen what we have just advanced, by comparing these cases with one in which the peritonæum was lacerated, with the substance of the uterus itself.

"A woman who had been violently pressed by a carriage against a wall, was instantly seized with violent pains in the abdomen, accompanied by a flooding, which continued for six weeks. After this discharge her health was not improved. She was in a state of constant and severe suffering. She became daily weaker, and was much-wasted. She was regular in her periods after the hæmor-



rhage ceased. She was taken to the Hotel Dieu the June following. She had had a violent vomiting for some time, which never ceased during the remainder of her life."

"When the body was opened, there was found in the abdomen a collection of a brown fœtid matter. The omentum was dissolved, and all the parts in the neighbourhood of the liver had suffered by inflammation and suppuration. Adhesions were formed throughout them. The uterus was in a natural state as regards colour, size and consistence; *but a rent was observed on its back part, which had never united.* In the midst of the adhesions the remains of a putrid child were found."\*

In this case the woman was seven months advanced in her pregnancy when the accident happened. She immediately suffered violent pain and had a flooding. She languished for about five months, (for the time is not strictly defined.) At the end of this time she died. Some time before her death she was seized with a vomiting, which continued until she expired. Upon opening her body "a collection of brown matter" was found. The "omentum was dissolved," and all the parts in the neighbourhood of the liver had undergone inflammation and suppuration, and had contracted adhesions with each other. The remains of a putrid child were found amidst the adhesions. The uterus, as to colour, size, and consistence, was in a natural state; but a "rent was observed on the back part, which had never healed." Here we at once perceive the effects of an extravasated fluid within the abdomen. From the moment of the accident there was a train of untoward symptoms, which could readily be traced to their source. Inflammation, with all its unpleasant consequences, was the result of a quantity of blood, &c. thrown upon the intestines, and ultimately death.

This is but too frequently the end of this accident; it is confessedly one always of extreme danger, though not uniformly fatal. But we are not in possession, as far as

\* Journ. de Med. 1786.

our information extends, of a single fact, where violent and alarming symptoms have not followed from the sudden presence of any extraneous body whatever within the abdomen. It would seem indeed that the powers of the system are sometimes capable of overcoming the consequences of certain fluids thrown into the peritonæal cavity; hence we find that blood, and even the liquor amnii, have not always proved fatal, when they have been discharged into it. But is there a solitary instance of a solid body, like that of the fœtus, having been suddenly immersed in the intestines without the most fatal result following? We think not.

In the case just related, the conservative power of the system was immediately called into requisition; but it was unavailing. In no case, perhaps, was this power more beautifully and conspicuously manifested than in the case in question. It had, by enveloping the fœtus in the folds of the adhesions, greatly diminished the irritation arising from its pressure and motion, and succeeded in obviating the mischief arising from its presence in the abdomen, as far as it was possible to succeed under like circumstances. We think, then, that this case decidedly proves, by its consequences, that, in the former, the fœtus had never penetrated the peritonæum.

To illustrate our position, we selected one of the most favourable cases\* we could find upon record for the adverse opinion; yet it was found that the system yielded ultimately to the disturbance produced by a fœtus of small size, and which we have great reason to believe never had penetrated the peritonæum. If then the powers of the system, after struggling for some months for mastery, be incompetent to the repair of the mischief occasioned by the presence of a small fœtus, how much less able would it be to overcome the consequences, when that body shall be of much larger size? Dr. Sims's case proves the same thing, p. 16.

\* The one from Jour.de Med.

It may be urged, that in cases of extra-uterine fœtuses, the peritonæum does not suffer from the presence of these bodies, as they are sometimes retained very many years. This we are not disposed just now to dispute ; but between the cases there is not the slightest analogy. In the case of an extra-uterine conception, the ovum is so gradually developed, and of course the peritonæum is so slowly, indeed we may say, so imperceptibly distended, that it would not amount to irritation ; and consequently the system would not for a long time be roused to notice its encroachments, and may therefore, as already admitted, be carried many years without any serious inconvenience. We have a strong analogy to support this explanation, in the formation of the abdominal dropsy. The fluid constituting it is so silently and gradually deposited within this cavity, that the peritonæum is not urged by its effects, to notice its presence. It quietly, and for a long time, submits to be greatly distended, without manifesting that it is annoyed by it.

That it is owing to the very slow progress of the ovum, on the one hand, and the very gradual deposition of fluid, on the other, that the peritonæum does not feel much inconvenienced by their presence, will be admitted, we trust, by every unprejudiced mind ; and we believe that it will be acceded to by an equal number, that, if a fœtus of a very small size, or the one twentieth part of the fluid which may constitute a dropsy, were suddenly thrown into the abdominal cavity, that the most serious and alarming symptoms would immediately ensue.

The peritonæum is never, we believe, lacerated in the human subject, with entire impunity : and the more especially where the wound will admit the external air into the abdominal cavity. We must therefore necessarily regard the rupture of either the uterus or vagina as an accident of extreme danger. This we are fully prepared to admit ; but this concession does not amount to the abandonment of all hope of recovery from these accidents ; for we are fully persuaded that there is no mistake in the cases related by the

respectable authorities we have quoted. And we are further persuaded that we should have had more instances of recovery upon record, had these cases of ruptured uteri been judiciously treated, or had they always been under the control of those who would have acted with that promptitude the exigency required. For there is a moment to act which will give at least a prospect, nay a probability of success ; if this be lost, every exertion may be unavailing. But as this *proper instant for action* is perhaps not indicated by symptoms, we shall always, we believe, be justified in the attempt to relieve, however unsuccessfully the case may eventuate. In Dr. J. Hamilton's case,\* he declares "almost every circumstance was unfavourable," yet the patient recovered ; while that of Dr. Gartshore† died after languishing twenty three days, with a variety of symptoms, "sometimes alarming, at others encouraging."

Danger, in cases of the kind we are considering, is not always commensurate with the apparent extent of the injury. Hence the fatal termination in cases where the injury appeared slight ; and complete recovery where hope was almost instantly abandoned. Now, as we cannot, *à priori*, determine the extent of power in the system, nor the degree of injury it may have sustained, we should act as if it were competent to the exigency, by removing every obstacle which might oppose its efforts. It would seem that the peritonæum at times will not bear the slightest injury without the most alarming consequences immediately following ; nay, death itself has taken place where the lesion of this membrane has been but slight, and entirely confined to itself. At other times, it would appear to suffer, without much inconvenience, extensive lacerations. In proof of the former assertion, we will briefly relate a case given by Mr. C. M. Clarke, in the *Medical and Chirurgical Transactions*, vol. iii. p. 290. "A woman between the age of twenty and thirty, fell into labour with her first child, about eight o'clock in the morning, under the care of a midwife. Her labour pains came on gradually ; but about ten o'clock

\* MS. Lectures.

† Douglass's Essay, p. 29.



she was suddenly seized with great pain in the belly and nausea, which did not end in vomiting. Great irritability succeeded, with faintness and excessive restlessness : and at half an hour past ten she died undelivered." "On the following day the cavity of the abdomen was inspected." "The uterus was in some measure contracted." "I gently turned the fundus of the uterus over the pubes with my hand, so as to bring into view its posterior surface. In the fold of the peritonæum which dips down into the pelvis between the uterus and the rectum, I observed above an ounce of blood ; and upon that part of the peritonæum which covers the posterior surface of the uterus, there were between forty and fifty transverse lacerations, none of which were in depth above the twentieth of an inch, and many were merely fissures in the membrane itself. They varied very much in length, some measuring one inch, some two inches, whilst the length of others did not exceed the fourth part of an inch. The space upon which they were situated extended from one side of the uterus to the other, and occupied the greater part of its whole posterior surface. The edges of these lacerations were thinly covered with flakes of coagulated blood, and there could be no doubt that the blood found in the peritonæum had escaped from these lacerations. The muscular part of the uterus was perfectly whole." In this case the appearances on dissection could scarcely be called sufficient to account for the death of the patient, yet in every other respect the body was declared sound.

In support of our latter position, we shall relate the history of "Elspet Grant,\* who, in April, 1736, took her labour pains. After they had continued three days, with the child in the birth, two cracks, as if the rafters had broke, were heard about the sick wife, and her belly was rent from near the navel, with a squaint downwards and to the left side, to near the share-bone. At this rent the child came into the world, the after-birth was brought away, and the entrails were seen."

\* *Phys. Essays*, vol. ii. 338.

“The rent was cured without any other application than that of butter mixed with white sugar.” This case is certified upon the oaths of a number of witnesses of the fact. These cases we think prove that the event alone is to determine the extent of the injury, and they hold out to us a strong inducement to act in all cases as if success were certainly to crown the exertion.

Mr. Burns\* has reiterated the sentiments of Dr. Hunter, Dr. Denman, and others; and as his work has had considerable circulation in this country, the pernicious maxim of “doing nothing” has consequently become more extended. He says,† “to leave the case to nature, like an extra-uterine pregnancy, is most likely to be successful.” We must again ask the question, Has Mr. Burns ever known a case terminate favourably that was thus treated? We repeat the question for the sake of information, as well as to insinuate our full persuasion that this has never happened. If Mr. Burns was in possession of any facts of this kind, it was his duty to have made them public, either in detail or by reference, neither of which has he done. His assumption of the position just quoted, like those of Dr. Denman, is entirely gratuitous; and highly calculated to do injury to the best interests of humanity, and to the improvement of the profession.

From all then we can learn, either from the experience of others, or our own, we are forced to conclude, there is no solid reason for withholding succour from the languishing patient, though our aid may be again and again unavailing. For what can we promise ourselves by a contrary conduct? Since, as we sincerely believe, it is not within the experience of any man, “that a woman has recovered from a rupture of the uterus when the child at or near the full term of gestation, has been suffered to remain within the cavity of the abdomen.” We have endeavoured to prove that certain cases purporting to be of this kind, are not entitled to this distinction; and consequently, must not be cited as such. In no department of

\* Principles of Midwifery, p. 158.

† Id. p. 178.

science, are false facts so mischievous as in the medical ; the perpetuation of one important error may have a thousand victims. And we are not less sincere in our declaration than honest in our intention, when we affirm, there is none of higher grade than that "a patient who has suffered a rupture of the uterus, has a better chance of recovery, by resigning her case to the natural efforts of the constitution, than by any operation or interposition of art."

But Dr. Denman is at variance with himself, for he expressly tells us, "besides some few others, of which I have been informed, or which are recorded, a case has occurred to my very *worthy, able, and experienced friend, Dr. Andrew Douglass*, in which the uterus was ruptured, he turned the child, the patient recovered, and had afterwards children."\* "If no other case," continues the Doctor, "had ever occurred, I apprehend this would be sufficient authority, to render it in future the indispensable duty of every practitioner to act in a similar manner ; and bad as the chance is of the patient, to be strenuous in using all the means which art dictates, to extricate her, if possible from her danger, or to preserve the child."

We may ask what has changed Dr. Denman's sentiments on this head? As for ourselves we are totally at a loss to imagine, though he gives a case in the beginning of his essay which he declares was the cause of it—but we see nothing in this case which should have had this effect ; for it is a case of common features, and from which no deduction, in our opinion, can be drawn, either favourable or unfavourable to the subject in question. If the declaration of Dr. Denman was strictly true, that, "there are more instances upon record of recoveries of women who have not been delivered, than of those who have been delivered, after rupture of the uterus," it would merit a most serious consideration ; but as we are convinced ourselves, and have endeavoured to prove that this is not the case, the Doctor's arguments have not in the least changed our opinion. But let us suppose that this

\* Introduction to Midwifery, vol. ii. p. 117.

is really the case, should it influence us to withhold our assistance from the poor woman when the argument is not fairly stated? For we must insist, that a vastly greater number of women have been suffered to remain without an attempt to deliver them after the child has passed into the cavity of the abdomen, than have been delivered; consequently the proportions not being equal, the inference must be incorrect.

Besides, Dr. Denman\* attempts in a most disingenuous manner to throw suspicion upon the case of his "*worthy, able, and experienced friend, Dr. Andrew Douglass,*" by declaring there was "no instance within" his "knowledge, except one, *which was doubtful*, of either of them (mother or child) being preserved." Why the case of Dr. Douglass should be doubtful in the year 1810, when it was recorded as an unequivocal instance of recovery in 1795, is extremely difficult to tell—it looks too much like a subterfuge, to avoid the force which the fact brings with it.

La Motte,† who had not the advantage of the experience of others, like Dr. Denman, to direct him, thought much more correctly upon the subject; and in the only two cases which occurred in his practice, he delivered, and solaced himself with the reflection, "*quelqu' inutile que fût cet accouchement, nous fumes plus contents tous deux, elle d'être accouchée, parce qu' elle en mourut plus tranquillement, et moi de l'avoir exécuté.*" His experience had not furnished him with a successful case, nor had the practice of any one else. He considered the case as hopeless, and was only prompted by the feelings of humanity to give the aid he did; and we see he was satisfied with himself for it.

If we may add our own experience on this subject, it would bear testimony in favour of delivery, where even no question but that of humanity could arise; for we are convinced that we diminish by it, both bodily pain, and

\* Essay on the Rupture of the Uterus, p. 7. † Traite des Accouch. p. 464.



that extreme anguish of mind, with which the patient is harassed. Sufferings that need not excite surprise, when we take a view of her unhappy situation; an extraneous substance of great bulk, solidity, and weight, to say nothing of the fluids discharged with it, is thrown suddenly upon the naked and exquisitely tender intestines, disturbing their order, interrupting their functions, and inevitably stimulating them to inflammation and all its terrible consequences. To this we may add, in many instances, for a time at least, the struggles of the fœtus itself.

As a moral question, it must, we think, be decided against the advocates for "doing nothing;" for there can be no doubt that in many cases, the woman's life is protracted by delivery, and consequently we have no right to abridge it, by the plea that "we are unwilling to give additional pain, where there is already but too much suffering." But if it be decided (and of this we entertain not the smallest doubt) that the woman's life is lengthened by delivery, we shorten it, if we withhold this benefit; and murder itself might, in many instances, be divested of its criminality, if the plea should be admitted, that its victim could have lived but a little time more. It is not for us to limit the sanative power of the system by theorising upon it; it is our duty to give every facility to it, by the removal, as far as may be in our power, of every obstacle that may oppose its progress towards a cure. In doing this, we but *obey* the dictates of "*reason and humanity*;" and should our efforts prove successful, however seldom, they would be amply rewarded. Facts alone should direct us upon this subject; and they, in our opinion, emphatically declare in favour of delivery.

It would be highly interesting, were it strictly practicable, to run a parallel between the cases where delivery was effected, and those where the woman was suffered to die without it. We are persuaded we should generally find the woman to have lived longer, and in some instances much longer for it; this alone, if a fact, would claim a serious consideration. If we are to judge from the in-

stances furnished us by the records of medicine, (and we have not at present any other mode by which we can determine,) and take for granted that the delivery was the cause of the protraction of death, we should, without the smallest hesitation, decide upon it as the preferable mode of practice; but as these cases may be urged as mere coincidences, we shall not insist upon them as proofs, as we wish this matter to be settled upon as solid a foundation as the nature of the case will admit. We shall therefore not consider the deductions from them as absolutely conclusive, though highly probable and strongly tending to corroborate our general position. In estimating the period of death from the moment of the rupture, (at least as certainly as that period can be determined,) it will be found that in almost every instance, the time from the accident to that of death is longest where delivery was resorted to. We regret we cannot, at this moment, have a more extensive range for comparison, but as no case shall be concealed that we are in possession of, we trust the inference will be fair from the data, if not entirely convincing. We have bestowed considerable labour to collect facts upon this subject, and we feel that we have estimated their force without partiality.

We have examined nearly all the cases collected by Dr. Douglass by referring to the authorities, and find them faithfully related; we shall make use of these, with others furnished by our own reading, for the purpose just proposed. We shall first give those cases where delivery had taken place; and then will follow those where the women died without being delivered.

	Lived after delivery.
La Motte's first case, <sup>1</sup>	3 days
second case, <sup>2</sup>	4
Garthshore's, <sup>3</sup>	23
Peu's <sup>4</sup>	8

1 Observ. 312.

2 Observ. 313.

3 Douglass, p. 35.

4 Pract des Accouch. p. 341.

Steidele's, <sup>5</sup>	- - - - -	7 days
Deweers's <sup>6</sup>	- - - - -	5 hours
James's <sup>7</sup>	- - - - -	a few minutes
Douglass's <sup>8</sup>	- - - - -	17 hours

## Cases undelivered, with their periods.

Monroe's case, <sup>9</sup>	- - - - -	2 days after rupture
Garthshores's (as quoted by Douglass, p. 35 of Essay,)	- - - - -	2 days after rupture
Pouteau's <sup>10</sup>	- - - - -	36 hours probably*
Saviard's, <sup>11</sup>	- - - - -	but a few hours probably
Denman's <sup>12</sup>	- - - - -	16 hours probably
Smellie's <sup>13</sup>	- - - - -	14 hours
Hooper's <sup>14</sup>	- - - - -	12 hours
Chatard's <sup>15</sup>	- - - - -	4 days, or as many hours ; there is great uncertainty in this case ; most probable but a few hours.

From the foregoing statement of an equal number of cases of each kind, taken precisely as they occurred in the search, we find that those women who were delivered, lived much longer, on the average, than those who were not delivered ; and though we are willing to admit this is not strictly proof, yet we must think it is rendered highly probable that the delivery of the child suspended the moment of death. If then it is highly probable that the life of the patient is prolonged by our efforts, it becomes an indispensable duty to make them ; but if we add to this, that there are many well attested cases of recovery where the child has been extracted, and not one to prove the opposite position, there cannot, or rather should not, be a moment's hesitation about the plan to be pursued.

5 Observ. de Rup. Uter.

6 Coxe's Med. Muc. vol. ii.

7 Med Repos. vol. i. p. 325.

8 Essay, p. 40.

9 Essays Phys. and Lit. vol. i. p. 339.

10 Œuvres, p. 487.

11 Observ. xxv. p. 131.

12 Douglass's Essay, p. 40.

13 Midwifery, vol. iii. p. 385.

14 Mem. Med. Soc. of Lond. vol. ii. 118.

15 Med. Repos. vol. vi. p. 128.

\* I have put probably to several, as the hours are not exactly mentioned.

2.—The causes which may tend to a rupture of the uterus are much multiplied by authors. Some, in our opinion, are wholly inadequate to the end; while others of powerful agency are slightly passed over. We shall not pretend to enumerate all that are assigned, but will notice some of the most prominent.

It has been supposed by many, that the force exerted by the child against the parietes of the uterus might occasion its rupture. La Motte, we believe, was the first who suggested this cause; and considering the few opportunities he had to observe this complaint, his opinion is both natural and respectable. He says,\* “Lorsque l'accouchement, s'est déclaré par de légères douleurs, qui sont devenues très violentes, les membranes qui contiennent les eaux s'ouvrent, et l'enfant y joint ses efforts, étant dans une bone situation, et ne se trouvant point d'obstacle qui empêche sa sortie, c'est une chose bientôt finie : mais si au contraire quelque chose se trouve qui l'arête au passage, comme une tête trop grosse, et les os ilion, ischion, et pubis, par trop serrez, c'est une nécessité que les violens efforts que cet enfant fait, réfléchissent contre le fond de la matrice, qui ne se trouvant pas toujours d'une égale consistance, ni assez forte pour résister si longtems aux impétueuses saillies de l'enfant ses parois sont à la fin obligez de céder et de se rompre.” Of this opinion was also Levret, Crantz, and some late writers; but we are perfectly of the opinion of Baudelocque, that “the child is almost always passive at the time the uterus tears. If it becomes the instrument of the rent, it acts no otherwise than any other solid body of the same volume, inanimate and of an angular surface would, on which the uterus should be strongly contracted.” We may add, in confirmation of this suggestion, that the uterus has been ruptured after the death of the child.

Dr. Denman says,† independently of disease, the uterus may be worn through *mechanically*, in long and severe la-

\* Traite des Accouchemens, p. 462.    † Introduction to Midwifery, p. 105.



labours, by pressure and attrition between the head of the child and the projecting bones in a distorted pelvis, especially if they be drawn into points or a sharp edge." Can it be imagined that the cause here assigned can act in the manner Dr. D. supposes? Can the friction or "attrition" spoken of exist to the extent necessary to produce the effect? We think not: for before the membranes are ruptured the head of the child cannot be so firmly pressed against any resisting point as to produce this "attrition;" for the instant it comes in contact with it, it will recede, in consequence of the liberty it enjoys amidst its waters; and after the liquor amnii is evacuated, the uterus embraces the fœtus so firmly in every point, that the mechanical action called "attrition" cannot take place. Besides the smooth surface of the uterus and the soft one of the child's head might pass over each other successively a thousand times without wearing the uterus through. Again, action and re-action being equal, the scalp of the child should be abraded as well as the uterus. Now this condition of the child's head has never, so far as we have heard, been noticed.

Burns says,\* that "in most cases it is owing to external violence." He must, we think, mean, where this accident takes place before the full term of utero-gestation, and not during the period of labour. In this, perhaps, he may be right; but if we compare the whole number of cases which medical writings furnish us with, we shall find that those which happen during labour are much more numerous than the others, and have not "external violence" for their cause. He indeed does not appear to be aware that he has given this opinion, for in page 267 he says; that "the most frequent cause, however, of this accident is a disproportion between the size of the head and the capacity of the pelvis, by which a portion of the cervix uteri is pinched between the head and the pelvis, and fixed so that the action of the uterus is directed against this spot rather than the os uteri."

\* Principles of Midwifery, p. 159.

This explanation is totally unintelligible to us. We cannot see why the powers of the uterus should be more forcibly directed towards that portion of itself which "is pinched between the head and the pelvis," or if they were, why this should occasion it to tear sooner than it would the os uteri, when directed against it, unless an important change in the condition of the part thus impinged upon had taken place; for until this change happens, (of which we shall speak presently) this part is as strong, *cæteris paribus*, as any portion of the uterus; and when it does take place, it gives way, not because "the action of the uterus is directed against this spot rather than the os tinæ," but because it has become the weakest part, and cannot now sustain the common efforts of the other portions of itself."

Mr. Burns says\* that "Salmathus, considers a thinness† of the uterus as a pre-disposing cause of rupture; and Dr. Ross‡ relates a case where it *seemed* to have this effect, the womb not being above the eighth part of an inch in thickness, and tearing like paper." We cannot well determine on what principle Salmathus supposes a "thinness" of the uterus to be "a pre-disposing cause of rupture," since we have no evidence of this state existing before the rupture has taken place, but as an accidental or temporary condition; and then perhaps, it is rather an imagined than a real occurrence. Certain it is, we have no way to ascertain the fact previously to the accident it is said to be the cause of, and a post mortem examination will not permit us with certainty, to declare that this "thinness" existed as an original structure. That the uterus is found thin upon dissec-

\* Principles of Midwifery, p. 268.

† By "thinness" we presume is to be understood a departure from the natural and ordinary thickness of this organ, as an original conformation of it; for if it were an accidental or temporary condition, there must be some cause to produce it; and that cause operate in such manner, as to stretch the parietes of the uterus beyond their usual degree of tenuity. In this case the "thinness" is but the effect of a preceding cause, and this "state," we suppose, must always exist where there is a powerful distracting cause, or in other words, that the uterus always becomes thin before it is stretched to bursting; but that it does not always burst by being stretched.

‡ Annals of Medicine, vol. iii. p. 277.

tion, in some instances of rupture, we cannot doubt; but that that was the state of the uterus before the rent, and existed as an original confirmation, we have in no case a proof; nor does Dr. Ross's case, as quoted by Mr. Burns, countenance any such belief. Dr. Ross does not give it as a cause of the accident which killed his patient; he merely mentions the fact without either comment or conjecture. That the uterus was unusually thin and tender we do not pretend to dispute; but we certainly would deny, without further evidence, that either or both of these circumstances were the cause of the rupture. Its "thinness" was owing, in our opinion, to a cause by no means uncommon in these cases, namely, an excessive hæmorrhage, which so effectually emptied the uterus of blood, that it could no longer maintain its accustomed thickness at this period; for there is no question now how the uterus supports this thickness; it is universally admitted to be from the augmented capacity of its vessels. It therefore follows as a consequence, that when exhausted of blood by the emptying of these vessels, its thickness must be diminished in proportion to the reduction of their calibers,\* provided the uterus has lost its power of contraction, or is mechanically distended by any agents within its cavity. Its tenderness was owing to a pretty advanced stage of putrefaction; for Dr. Ross says that the uterus was "emphysematous," and "was exceedingly distended, and of an uniform figure, as if blown up." And this condition favoured the further distension of the uterus, and rendered it, most probably, much thinner than at the moment of the rupture.

"Mental agitation" and "frights" are said to produce the rupture of the uterus. Of this we entertain strong doubts. When these do act upon the uterine system, it is, most probably, only to excite labour prematurely. This,

\* That this is the true explanation of the "thinness" of the uterus is corroborated by a case related by Smellie, vol. iii. p. 361. The woman died of flooding, and he says the "uterus and other parts of the woman's body seemed to be quite destitute of blood, for scarce a drop appeared on opening the parts," and the uterus "was not a quarter of an inch thick." We shall also give a case which fell under our own observation tending to confirm the same explanation.

it is true, may be followed by a rupture of the uterus, but we believe nothing more. Dr. Sims's case was one which was preceded by "great agitation of mind," but we have every reason to believe that the rupture did not take place for two months after the first effects of this "agitation" "and a very long walk" had manifested themselves in the production of "an uterine hæmorrhage."

Having noticed in a brief manner a number of circumstances purporting to be causes of the rupture of the uterus, and offered a few remarks upon each, to show our reasons for not admitting them to be such, we shall now proceed to consider those causes which must be allowed as adequate to produce this effect. They may be conveniently divided into two kinds: those which act directly, and those which act indirectly upon the uterus.

The first or direct, are mechanical violences, and may be external or internal. The external may be a blow,\* a fall,† a kick or violent pressure;‡ the internal may be, attempts to turn§ or to return a prolapsed limb,|| or the maladroitness of application of instruments,¶ or the unequal surface the fœtus itself may present.\*\*

The second, or indirect are those which impair the integrity of the substance of the uterus, such as all those causes which offer a mechanical impediment to the passage of the child, as a contracted pelvis,†† an unusual sharpness of the linea iliopectinea‡‡ and exostoses,§§ tumours,||| scirrhus indurations,¶¶ and ulcers.\*\*\*

The first set of causes act directly by exerting a force beyond the resisting power of the uterus itself; the second by diminishing the strength of some particular portion of that viscus, so that its own contractile powers are sufficient

\* Journ. de Med. 1780.

† Journ. de Med. 1780.

‡ Annals of Med. p. 278.

\*\* See Mrs. M's case of this essay.

†† Perfect's cases, p. 448.

§§ Med. Mus. vol. ii.

¶¶ Perfect's Cases, vol. iii. p. 439.

† Hamilton's MS. Lectures.

§ Dease as quoted by Burns.

¶ Dr. Hunter, Med. Journ. vol. viii. p. 368, as quoted by Burns, p. 481.

‡‡ Burns's Prin. Mid. p. 268.

||| Baudelocque, vol. iii. p. 413.

\*\*\* Baudelocque, vol. iii. p. 413.



to overcome the resistance which the injured part offers to them when strongly excited.

The operation of the first set of causes is sufficiently obvious without any explanation; and the influence of the second is by far the most common, though not so immediately evident. When there is a contracted pelvis, the head of the child is prevented from freely engaging in it; and as it is always covered by the uterus at the commencement of labour, and in most instances until the last period, it follows as a consequence, that it must be confined between the presenting part, and some portion of the pelvis; now, if it be too long retained in this situation, and more especially if it rest against a sharp edge, as the linea iliopectinea,\* or an exostosis,† inflammation will ensue, and if the cause be not removed quickly, run on to gangrene. When this has taken place it is easily understood how rupture may take place at the part thus injured, even without any extraordinary exertion from the healthy portions of the uterus.

The second set act, by preventing a regular distension of the uterus, during gestation; of consequence, some one portion or other is unduly put upon the stretch, and of course weakened—and by being passive during labour, from its diseased condition, cannot resist the efforts of the healthy portions.

Baudelocque‡ says, “the violent and sometimes convulsive action of the uterus on the child’s body, is almost always the cause of its rupture.” But that “the rupture in question cannot happen in any case, unless the uterus has been pre-disposed to it by those means (those we have just enumerated) or by other causes which are all accidental.” It follows, then, that if every point of surface of the uterus, is equally disposed to support the efforts made to expel the child, the rupture cannot take place; but if any one part is weakened from any cause whatever, that portion ne-

\* Burns, p. 268.

† Dewees’s case, Med. Mus. vol. ii.

‡ System, vol. iii. p. 412 and 413.

cessarily runs the risk of laceration. When the action of the uterus itself is the remote cause of rupture, the latter always happens at the moment when the former is exerting its highest power; hence this takes place in the height of a pain. This accident may happen to any portion of the uterus or at its connexion with the vagina; and the rent or tear may be in almost any direction. It may be more or less extensive, so that the child with its appurtenances may escape entirely into the cavity of the abdomen, or only part of it, or it may remain confined to the uterus. These different conditions are not alike unfavourable: when the child leaves the uterus altogether or partially, the woman's chance of recovery is much less, than where it does not pass through the wound, for a mere lesion of the uterus itself is not necessarily fatal. It is the injury done in the abdominal cavity by the presence of a foreign body that chiefly constitutes the danger.

This accident, when it happens, is almost always announced by very decided symptoms, which shall now be considered under the third division of our subject.

3. It has been supposed by some, that the rupture of the uterus was preceded by symptoms which pretty distinctly announced this event to be at hand. Of this opinion were Crantz and Levret, agreeably to Baudelocque: Crantz says,\* that "when a woman is threatened with a rupture of the uterus in a laborious labour, the belly is very prominent and tight; the vagina lengthened, and the orifice of the uterus very high; the pains are strong, leave little interval, and do not advance delivery." Even these latter symptoms, though much less equivocal than those which precede them, are by no means certain; the case of Mrs. M., which we shall soon relate, will prove that an extensive rupture may take place, where the circumstance of "strong pains, with little interval," was not among its precursors. To these signs M. Levret† adds, "that the pain the woman suffers, is always seated towards the mid-

\* Baudelocque, vol. iii. p. 420.

† Ibid.

dle of the epigastric region ; that a last effort or violent leap succeeds to the repeated strugglings of the child, which announces its death and the rupture of the uterus." Most of the signs just enumerated, belong to almost every case of " laborious labour," especially the prominence and tightness of the belly ; these appearances take place almost necessarily from the condition and relation of the parts to each other ; for in almost all cases of this kind, the waters are drained off, sometimes earlier and sometimes later ; but in proportion to the time, will be, *cæteris paribus*, the force with which the uterus will employ its tonic power of contraction, and this power accommodates the parietes of the womb, almost strictly, to the varied surface which the fœtus presents ; this then accounts for the tightness ; and the prominency is entirely owing to this same power bringing the presenting part immediately over the opening of the pelvis, which it is obliged to enter, if it enter at all, at an angle of at least thirty-five degrees, consequently the fundus must be thrown in an equal degree forward, that its axis shall correspond with that of the superior strait ; and this arrangement constantly takes place, and the uterus is equally " prominent and tight" in every case of labour where the waters have been drained off for any length of time ; yet a rupture is comparatively of rare occurrence ; on the other hand, when the waters are not discharged, this prominence and tightness cannot take place, yet there have been instances of rupture while the membranes have remained entire.\* As regards the length of the vagina, we know this must vary ; for it must be long or short, as the presenting part may be far or near from the os externum, for when there is a narrowness of pelvis either absolute or relative, or an anterior obliquity of the uterus existing, there the vagina must necessarily be " lengthened," yet rupture is very far from happening whenever either of these circumstances obtain ; while on the other hand we know that rupture has taken place when

\* Smellie, vol. iii. p. 385 ; also Mem. Med. Soc. vol. ii. p. 118.

the head of the child has been low in the pelvis,\* and consequently the "vagina" very short.

The sign which Levret has added to those indicated by Crantz is equally doubtful ; for instances of rupture have occurred where there was no pain in "the middle of the epigastric region," and many times none has followed this symptom ; and it is a fact long since upon record, that the uterus has yielded, after the death of the child.† We therefore perfectly coincide with Baudelocque, "that the rupture of the uterus has often taken place without being preceded by any of them, and has not happened in other cases where their union declared it inevitable;"‡ and most heartily concur in the opinion, that "if we were to take them for our guide, we should sometimes trench upon the rights of nature, by performing a delivery which she would have been able to terminate without inconvenience."§

Dr. Douglass|| in reference to Mrs. Manning's case, which he has so circumstantially and clearly related, and from which she most fortunately escaped, says, "I shall ever retain the most lively impression of the nature and appearance of the poor's woman's throes ; of the agonising sensations they seemed to excite in her, and the little effect they had in propelling the child. These, with a recollection of the alarming accident of the rupture of the uterus which followed, would amount to a degree of internal evidence of what I had to apprehend, which I should not think myself at liberty to reject." And confesses "that was another case to occur, resembling in most of its previous circumstances" that of Mrs. Manning's, he "should be tempted to turn and extract by the feet."

We do not mean to interfere with the conduct of any gentleman under the apprehension of rupture of the uterus ; as we presume under so trying a circumstance, every one would act most scrupulously in the way he thought best ;

\* Douglass's Essay, p. 50.

† Annals of Med. vol. iii. p. 293 and 303.

‡ System of Mid. vol. iii. p. 421.

§ Ibid. Loc. cit.

|| Essay, p. 47.



nor should we condemn any man for an error in judgment ; yet we should be very far from recommending as a rule of practice the one suggested by Dr. Douglass, where there was a strong combination of symptoms which might seem to announce this event at hand ; for who by a forced delivery, can flatter himself that he has prevented the uterus from lacerating ? Could moral certainty be arrived at by the attending symptoms, our profession would gain much ; we could then prevent the rupture, and most probably secure the life of both mother and child ; but as this certainty cannot be commanded, and as threatening cases are very common, and the accident comparatively very rare, we cannot feel justified in recommending artificial delivery on the mere presumption of a contingent advantage. We would be the more cautious on this head from the consideration, that, wherever this might be thought expedient from the symptoms, there would also be a risk of producing the accident, the operation purported to avoid ; the waters long drained off, the uterus contracting strongly, and firmly embracing the body of the child ; the latter very large, or the pelvis contracted, offer difficulties which the inexperienced can neither appreciate nor anticipate, and the experienced would dread to encounter ; and should the practitioner dare the enterprise, and, by perseverance on his side, and resolution and suffering on the part of the patient, achieve the delivery, can he solace himself, or honestly assure the woman that he has saved her a ruptured uterus ?

We will now relate the case we have hinted at above, as it contains several peculiarities ; the patient was originally under the care of my friend Dr. Samuel Stewart, whose account we shall employ until the period at which we were called upon to see her.

“ On the 4th or 5th of July, 1820, I was requested to visit Mrs. M——. She made no particular complaint, but expressed some uneasiness at having passed over a few days (five or six) the time she calculated she should have been confined ; a circumstance which had never occurred

with her before, although the mother of five children. I assured her those mistakes were very frequent and should cause her no uneasiness."

"On the 14th, the membranes ruptured and the water was evacuated without any previous pain; nor did any supervene till the morning of the 17th, when about ten o'clock I was sent for. The os uteri was very much dilated, and the vertex presented at the brim of the pelvis; the pains not being severe, and recurring at long intervals, I left her, with a request, that as soon as they became more frequent and severe, they should immediately send for me. About two o'clock they sent and stated that her pains were quite severe; fearing lest I should be too late, as some of her former labours had been rapid, I went immediately, but before my arrival the pains had entirely ceased. The os uteri was now fully dilated, and the cervix somewhat lower."

"Expecting every moment a recurrence of the pains, I waited for some time, but finding they did not come on, she was requested to walk about the room; and while walking, the form of the abdominal tumour was so particularly situated, that several females who were present remarked it; the tumour was less round, and more flat above, and acute in front than usual. This appearance, as I had not the slightest suspicion of the fact, I explained to myself by supposing the great laxity of the abdominal muscles had allowed the fundus of the uterus to fall more than usually forward, and the length of time since the waters were discharged had given the uterus full time to contract over some projecting part."

"Near an hour had now passed without any return of pain, and the pulse being in a state nearly natural, it appeared a fair case for the use of the *secale cornutum*; ten grains of it were given, and finding it to produce no sensible effect, after an interval of half an hour, ten grains more were exhibited; although she felt no regular pain, she complained of excessive uneasiness and distress, and repeatedly declared 'her child would kill her.'"

“Between seven and eight o’clock, still suspecting no danger, I again determined to try the ergot; this was given in the same quantity, at the same interval, and with the same inefficacy as before. It was now after nine o’clock; she felt herself becoming worse—her uneasiness was excessive, and her spirits almost exhausted, yet her voice was strong—her pulse was feeble and sinking—and now, for the first time, her case appeared alarming, and I began to fear something terrible had taken place, and Dr. Dewees was immediately sent for, but by the time he came, she was nearly or altogether pulseless, yet still retained her recollection with considerable strength of voice.”

When I arrived, the patient was in the situation stated by Dr. Stewart; I immediately conjectured the nature of the accident, and it was agreed that no other chance remained but from instant delivery. Dr. Stewart sat down for this purpose, and was soon in possession of the feet. At this moment Dr. James arrived, who had also been sent for with myself. He concurred in the measure adopted, and kindly waited the event. After the body of the child was delivered, great difficulty was experienced in bringing down the head. Dr. S. was considerably fatigued by his exertions, and I took his place: upon passing up my hand to adjust the position of the head, I found it of an unusual size, and that every attempt to deliver it whole would be in vain—it was agreed it should be opened, and Dr. S. went in search of the proper instruments; but before he returned, life had so far ebbed, that any other attempt would be useless—the body was separated from the head—and the poor sufferer quickly after breathed her last.

Leave was obtained next day to open the body—accordingly Drs. Chapman, Stewart, Horner, and myself went for the purpose of the examination, and Dr. Horner proceeded to the operation. On opening the abdomen, a large tumour of a globular form presented itself, which was the uterus, inclosed in which, was the severed head—a stratum of coagululum covered the superior part of the uterus—the uterus was thin and flaccid—in the abdomen was about three pints

of extravasated fluid blood. Upon raising the uterus and carrying it towards the symphysis pubes, a laceration was discovered, running from the vagina to the right superior part near to the insertion of the fallopian tube—the rent was entirely through the neck and body of the uterus, but did not extend into the fundus. The placenta maintained its connection with the uterus. The vessels of the large intestines were much injected—the peritonæum on the anterior of the abdomen was studded with red vessels—the bladder was sound—the lower part of the wound where the rent began, was gangrenous.

The head of the fœtus was removed from the pelvis, in which, from its size, it could not engage, and was subsequently subjected to examination by Dr. Horner, who has favoured me with the following results :

- “ The horizontal circumference of the superior part of the cranium measured . . . 1 foot 10 inches.
- “ Diagonal circumference . . . 1 11½ do.
- “ The cerebellum, of natural size and condition.
- “ Cerebrum devoid of convolutions,  $\frac{1}{4}$  inch thick.
- “ Lateral ventricles containing three pints of water.
- “ Bones somewhat larger than usual, and the sutures widely separated.”

This case is remarkable, first, in the entire absence of symptoms which would lead to the suspicion, that a rupture was about to take place. Second, in the perfect freedom from those marks which distinguish this accident after it has taken place; for there was neither vomiting nor even sickness—no fainting nor disposition to it—no frequency of pulse, nor hurried respiration. Thirdly, no particular event or expression decided the moment at which the uterus gave way—no exclamation from sudden and acute pain; nor any noise to characterise the injury—no external hæmorrhage—in a word, nothing to lead to the suspicion that a laceration had happened. Fourthly, that in this case there was neither “deformity of pelvis,” “exostosis,” nor unusual “sharpness of the linia iliopectonea”—no “tumour,” “scirrhus induration,” nor cartilaginous



condition of the os uteri"—"no external violence," nor "internal force" to account for it. But the dissection renders it probable that the uterus was predisposed to the accident before the period of labour; and it clearly demonstrated, that it was owing to the pressure of the child's head against the projection of the sacrum, inducing gangrene in that portion of the uterus which was included between the points of contact. But, strictly speaking, this might be considered as a case of relative deformity of the pelvis, as the enormous size of the head rendered the opening of the superior strait too small for its passage, so that all the effects that would arise from an absolutely diseased one was produced.

Having remarked upon the uncertainty of any sign that is supposed to be the forerunner of the rupture of the uterus, and said that when this accident happens, it is almost always announced by very decided symptoms; we shall proceed to enumerate those that almost uniformly attend, the moment after the lesion has taken place.

For the most part, the woman feels an acute pain at the part where the rent has happened—she generally shrieks out, and declares that something unusual has happened to her—the rupture is sometimes accompanied by a noise that is audible to the by-standers—a discharge of blood of greater or less extent is noticed from the vagina—her face becomes pale—her respiration is hurried—she becomes sick at the stomach, and most frequently vomits—the matter discharged is sometimes only the common contents of the stomach, at other times very dark coloured, and even black—the pulse becomes extremely frequent, small, fluttering, or extinct—the woman complains of a mist before her eyes, loss of sight, and extreme faintness—a cold clammy sweat bedews the whole body—and convulsions and death follow, if she be not speedily relieved.

It would seem that the symptoms are modified by several circumstances—first, whether it be the uterus itself, or its connection with the vagina that is ruptured—second, whether the child has escaped, either in part or wholly, into

the abdomen—third, whether the lesion not only passes through the substance of the uterus itself, but through the peritonæum also.

1. When the rupture takes place in either the neck or body of the uterus, pains, however brisk or frequent before, almost always cease altogether, or become of a feeble, transitory kind, that have little or no tendency to forward the child. The woman, for the most part, becomes more rapidly weak, either in consequence of the importance of the organ injured, or from the hæmorrhage that almost always attends this kind of rupture.

2. When the child escapes entirely into the cavity of the abdomen through the rent in the uterus, pain instantly ceases, and the most distressing and alarming symptoms are almost certain to follow. If it be but partly protruded, the pains may continue and even effect the delivery; or the child may be extracted without any very great inconvenience. But if so much has passed through, that the powers of the uterus itself, or the aid which art may give, be inadequate to the delivery of the child, we shall then have a train of as untoward symptoms nearly, as if it had entirely passed through.

3. Should the rent stop at the peritonæal covering of the uterus, we have reason to believe that the symptoms will not only be much milder, but that the woman's chance of recovery will be much greater.

Notwithstanding, however, the very decided character the symptoms attending rupture of the uterus assume, they are not exclusively to be relied upon; but they are calculated to rouse us to a painful suspicion; and we should lose not a moment to have them either removed or confirmed. This can only be done by a careful examination of the abdomen and of the uterus; the first by the application of the hand externally, and the other per vaginam. Should the accident occur before the rupture of the membranes, the tumour which they formed will shrink away, not again to return perhaps; for should the rent be through to the abdominal cavity, it is more than probable that the membranes

will yield their contents within it; but should the lesion be arrested by the peritonæum, they may remain entire for some time, though they may not again form a pouch within the circle of the os uteri: for this last will most probably contract pretty firmly after this event, although previously well expanded, and may, from this circumstance, serve to distinguish the accident.

If we apply the hands to the abdomen, we may expect to detect the fœtus within its cavity, if the rupture be complete, either by its stirrings if it has not parted with life, or by tracing its limbs through the thin parietes; in this case, we shall almost always find the contracted uterus occupying its usual place. Should the symptoms lead to strong suspicion that the uterus has given way, and we find it still maintain its globular form, we have a right to conclude that either our suspicions are not exactly confirmed, or that the fœtus is still retained within its cavity, though the rupture has taken place. But this is not to lull us into a security that may be fatal to both mother and child.

If the accident happen after the rupture of the membranes, the presenting part will either recede beyond the reach of the finger, or will be so easily forced back, (provided it has not absolutely engaged in the pelvis) as immediately to excite alarm, if not confirm suspicion. Under such circumstances we should not trust to the "touch" alone; the hand should be cautiously introduced into the vagina, and the most careful and deliberate examination be made. This examination will detect, not only the rupture, but the part that has sustained the injury; should it be the uterus itself, we shall be able, with little or no force, to pass the hand through the os uteri, if the accident has happened after the labour had been well advanced; and this would lead to the knowledge of the exact situation of the patient. But should the uterus have given way before the os tincæ was sufficiently dilated to pass the hand freely, we should not be tempted to use a force that might be as destructive as the accident we were dreading; we

may, however, profit by the situation of the hand, as it will enable us to pass the finger far, if necessary, into the uterus through its mouth, and by this we can ascertain whether the fœtus still entirely occupies it, for in this case it will be within a very small distance of the os uteri; if it has partially escaped into the abdomen, it will most probably be more remote; and if it be entirely in that cavity, it will no longer be within reach of the finger. We must obey the same rules should the rupture take place before the full period of utero-gestation.

But if the laceration happen to the neck of the uterus, or at its connection with the vagina, it is much more frequent that the fœtus with the placenta pass immediately into the abdomen; in either of these cases, the presenting part will suddenly and entirely remove from the superior strait; we are immediately to examine the patient, so soon as these symptoms render it more than probable that this event has taken place. There does not exist the same difficulty to a satisfactory examination in these latter cases as in the former; for when the uterus is lacerated at its neck, the wound for the most part is so extensive as to permit the hand to pass without difficulty into the abdominal cavity; as the orifice of the uterus is prevented from contracting by this rupture of its circular fibres; and when the rent takes place at the union, if we may so term it, of uterus and vagina, there is, perhaps, even less difficulty to the passage of the hand; for the wound cannot diminish in capacity in any direction by the contraction of any of its fibres, but would rather augment, if they possess such a power; the uterus will be found for the most part firmly contracted, either on the anterior or posterior portion of the pelvis, as it may happen to be either the anterior or posterior portion of the vaginal circle that has given way.

The intestines will frequently prolapse through the wound, which but too decidedly declares the nature of the accident; or we may encounter them immediately above the edges of the rent; and no one that has not experienced this trial, can possibly imagine the thrill of



horror with which he will be seized, the instant his hand comes in contact with the naked bowels. It is almost needless to suggest the propriety of a cautious and gentle examination, when the hand has entered the abdomen. The difficulties and danger which must necessarily accompany a laceration of the uterus, may be augmented by a portion of intestine being engaged in the wound, and there severely pinched by the contracting orifice.

4. Having noticed, as briefly as the importance of the subject would admit, the three first divisions of our subject, we shall now proceed to the fourth and last; in which we propose to consider "the mode of proceeding under all the various circumstances with which this accident may be complicated." From what we have said in the commencement of this paper, it will be perceived, that we have no reliance on the powers of nature to effect a cure, when the child has escaped into the abdominal cavity; and that we are of opinion, that nothing but the prompt and judicious interference of art can rescue the unfortunate woman from the impending fate with which a rupture of the uterus threatens her.

For the interference of art to be even *probably* successful, it must not be delayed a moment beyond the detection of the accident, whenever it is practicable to seize that moment; for we have no hesitation to believe that there would have been much fewer victims from this cause, had the practitioner been aware of the nature of the case, or sufficiently intrepid to have instantly acted, when he had ascertained the uterus was ruptured. The injury which the system sustains from this casualty, is not simply from the lesion of the uterus itself; but also from the additional evils which must follow from an inflamed peritonæum. This would seem inevitable from the very nature of the accident, and must consequently be augmented in proportion to the continuance of the exciting cause. To be useful then, we cannot be too early in the removal of the offending bodies from the cavity of the abdomen, and by this means abstract a powerful and never

ceasing stimulus; for we must repeat our conviction, that there never has been a recovery, where the fœtus, &c. were allowed to remain.

Believing then, we have said enough to convince an unprejudiced mind that the positions and conclusions of Dr. Denman, if not gratuitous, are certainly erroneous, we shall proceed to consider the methods which should be pursued under the various combinations in which they may present themselves, and may be arranged as follows:—

I. When the laceration is confined to the body or fundus of the uterus, but penetrates the peritonæum, and the child escaped into the abdomen; this may happen,

- a.* where the pelvis is well formed.
- b.* where the pelvis is deformed.
- c.* where the uterus contracts firmly.
- d.* where the uterus remains flaccid.\*

It may happen,

- 1. where *a.* and *c.* are combined.
- 2. where *a.* and *d.* are combined.
- 3. where *b.* and *c.* are combined.
- 4. where *b.* and *d.* are combined.

\* This case does not seem clearly recognised by Baudelocque, and makes him somewhat at variance with himself; for in § 2175, he says “for notwithstanding that accident,” (the rupture of the uterus) “it is not always impossible to extract the child by the usual passage. De la Motte and others furnish examples of it, which I do not quote to serve as models. The former turned a child, searching for the feet through the rent in the uterus, as far as the middle of the belly whither they had penetrated; and others assure us they had brought back a child that way, which had entirely escaped out of the uterus;” but adds immediately after, “which will appear not very probable to those who know how much the uterus contracts from the moment it is emptied, and how much the rupture then loses of its extent.” Though the uterus does, for the most part, close, as stated by M. B., yet we know from a number of cases which are upon record, and from our own experience, that this is not invariably the case; and as far as our observations extend, the uterus is wont to remain *flaccid*, if the rupture be accompanied by a considerable discharge of blood. Besides, the case quoted from La Motte, on which he seems to place reliance, was an instance in which the child had passed entirely into the abdomen. For La Motte, in his “Reflection” on this case, says, that the body was opened after death, and that “l’on ne trouva á la matrice que le vestige de cette ouverture, dans laquelle l’on ne put introduire que le bout du petit doigt, quoique le corps de l’enfant y eût passé tout entier.” La Motte Obs. cccxvii. See also Douglass’s Essay, No. vii. p. 29, No. viii. p. 31.

II. Where the laceration may pass only to the peritoneal coat, and the child not into the abdomen; this may happen;

- a.* where the pelvis is well formed.
- b.* where the pelvis is deformed.
- c.* where the uterus contracts firmly.
- d.* where the uterus remains flaccid.

It may happen, 1. where *a.* and *c.* are combined.

2. where *a.* and *d.* are combined:

3. where *b.* and *c.* are combined.

4. where *b.* and *d.* are combined.

III. Where the laceration is confined to the neck of the uterus and vagina, and the child, &c. have escaped into the belly; this may happen,

- a.* where the pelvis is well formed.
- b.* where it is deformed.
- c.* where the uterus contracts firmly.
- d.* where the uterus remains flaccid.

It may happen, 1. where *a.* and *c.* are combined.

2. where *a.* and *d.* are combined.

3. where *b.* and *c.* are combined.

4. where *b.* and *d.* are combined.

IV. Where the laceration is confined to the vagina alone, and the child in the abdomen; this may happen,

- a.* where the pelvis is well formed.
- b.* where the pelvis is deformed.
- c.* where the uterus contracts firmly.
- d.* where the uterus remains flaccid.

It may happen, 1. where *a.* and *c.* are combined.

2. where *a.* and *d.* are combined.

3. where *b.* and *c.* are combined.

4. where *b.* and *d.* are combined.

V. Where I. and III. are complicated with a descent of intestine which may happen,

- a.* where the pelvis is well formed.
- b.* where the pelvis is deformed.
- c.* where the uterus contracts firmly.
- d.* where it remains flaccid.

This may happen, 1. where *a.* and *c.* are combined.  
 2. where *a.* and *d.* are combined.  
 3. where *b.* and *c.* are combined.  
 4. where *b.* and *d.* are combined.

VI. Where the laceration may be either in the fundus, body, neck, or vagina, but the child remain either entirely or in great part in the cavity of the uterus; in these cases the head or presenting part may be

- a.* at the superior strait.
- b.* engaged in the superior strait.
- c.* or low in the pelvis.

These *a. b. c.* may happen,

- 1. in a well formed pelvis.
- 2. in one but moderately deformed.
- 3. where the uterus contracts firmly.
- 4. where the uterus remains flaccid.

VII. Where the laceration may happen either at the fundus, body, neck, or vagina; but where the fœtus, or the greater part of it, is still within the cavity of the uterus; and the presenting part above the superior strait.

These may happen, *a.* where the pelvis is much deformed.  
*b.* where the uterus contracts firmly.  
*c.* where the uterus is flaccid.

We believe we have exhibited in the above schedule every material variety that a lacerated uterus may present; and trust it will be found both clear and correct. We shall now proceed agreeably to this arrangement to point out the line of conduct which should be pursued in each particular division of the cases. In our attempts to relieve the patient under this afflicting event, we find ourselves restricted to three general modes.

First.—To attempt delivery per vias naturales.

Second.—To attempt it by gastrotomy or the Cæsarean operation.\*

\* On the subject of gastrotomy Baudelocque (Midwifery, vol. iii. p. 418,) makes the following judicious remarks:—"The fear of being charged with unskillfulness in announcing the rupture of the uterus, at the instant it happened, has hindered accoucheurs who have been witness of it, from employing the only



Third.—To attempt nothing, but leave the case to Nature.

The first mode is decidedly the one to which our feelings would yield the most ready assent ; but it is neither always proper nor even practicable. We must therefore sometimes be under the necessity of employing the second, if we mean our patient should profit by our aid. Or should we refuse it, we must abandon the unfortunate woman to the third. From what we have said, we shall be understood to decide that this latter should never be adopted with the expectation that success will attend it ; yet we may, from an imperious necessity, be obliged to follow it.

Gastrotomy and the Cæsarean section present horrors to the mind peculiarly their own ; nor should we be able to overcome the appalling sensations they produce, if we were not influenced by paramount considerations. To save life is a strong motive to the operation ; and to be snatched from death is a powerful inducement to submit to it. Where this is the only resource, the case should be fairly and candidly stated, that no after blame may attach ; and in all cases of such hazard, responsibility should be divided by requesting the concurrence of a brother practitioner, where time too precious would not be lost in this compliance. It has been called a “horrible expedient” by Dr. Douglass.\* It is so confessedly : so are lithotomy and many other operations : but this is not to be the test. Its utility alone ought to determine whether it should be

means possible of saving the mother and child, much more than the opinion they held that it was essentially mortal. M. Levret, who, as well as many others, thought that gastrotomy was the only resource in such cases, seemed to doubt whether it would ever be put in practice. The mother and child are inevitably lost, says he, when the uterus tears before delivery ; there is no means of saving them but the section of the abdomen performed instantly : but, continues he, what accoucheur would be bold enough to perform it in time, and what relatives would have courage enough to permit it to be executed without delay ? A great number of cases attest the truth of M. Levret’s prognostic, and there are some which no less demonstrate the necessity of recurring to the operation which he dared not recommend openly ; and show that there have been surgeons so regardless of their own interest as to propose performing it instantly, and that there have been women courageous enough to submit to it.”

\* Essay, p. 51.

considered as a resource of our art, or be for ever proscribed. For we are by no means satisfied with Dr. Douglass's reasoning upon this subject. He asks "if a rupture of the uterus is of itself an injury so generally fatal, what is the patient likely to gain by combining the dangers of such an accident with those of a penetrating wound which will expose the abdominal viscera?"\* Dr. D. seems to have forgotten that there was already "a penetrating wound" which "exposed" the abdominal viscera; and that an additional one through the teguments would scarcely enhance the risk, since we know that wounds of this kind are not necessarily mortal.†

Besides, what would Dr. D. have us to do in those cases where there is no possible alternative (as VII. *a.* or in I. 6.) but this operation? For it is only in cases similar to these that the operation is recommended. The woman can but die after the operation: and this she certainly will do if it be not had recourse to. And what practitioner would not prefer an alternative that may succeed though hazardous, to the abandonment of a patient to the unrestrained consequence of disease?

That it has been successfully employed we are not at liberty to doubt; nor is there any testimony that it has been either wantonly employed, or that it has added new sufferings or new dangers to the already almost certainly fatal disease, for which it is proposed as a remedy. We shall add the evidence we are in possession of, that it has been successfully performed, and from it allow every one to draw

\* Essay, p. 51.

† We find three highly interesting cases of the extirpation of the ovaria, in which there was a speedy restoration to health, although the wounds through the teguments of the abdomen were extensive, and its cavity a long time exposed to the air. In neither of these cases did any untoward symptom arise, though in the first case the tumour was so large as to contain fifteen pounds "of a dirty glutinous looking substance," and the sack which contained it, after being extirpated, "weighed seven pounds and one half." In the second, notwithstanding every care was taken to prevent it, a quart of blood was spread among the intestines, yet no unpleasant symptoms are said to have arisen. In the third, a diseased ovarium was taken out, which weighed six pounds, yet the patient recovered "in two weeks." Dr. McDowell's cases, *Eclectic Rep.* vol. vii. p. 242.

his own conclusions as to its advantages. As regards ourselves, we have no hesitation in believing that it is exclusively indicated in several combinations of ruptured uterus. Mons. Thibaut des Bois, a surgeon of Mans, has given an account of this operation having been successfully performed on a woman several hours after the accident, though too late to benefit the child. He adds, "that the woman suffered scarcely more than from the consequences of a common labour."\* M. Lassus† quotes a history of this operation having been twice performed with entire success on the same woman; and as the case is highly interesting, and the work from which it is taken is not much known in this country, we have translated it for the satisfaction of our readers.

"A woman of a strong constitution, of about thirty years of age, and pregnant for the fourth time, was seized with strong labour pains. One pain was so particularly severe as to occasion her to faint. Immediately after there was a discharge of blood from the vulva; the hand was introduced into the uterus, but the child was not found within it. The pains ceased, the faintings became more frequent, the extremities cold, and the pulse agitated. Convinced that the uterus was ruptured, and that delivery could not be effected in the natural way, the operation of gastrotomy was performed eighteen hours after the accident. In six weeks the woman was able to attend to her ordinary duties. She again became pregnant; and on the 30th December, 1779, Mr. Lambron was again called to her aid. When he arrived, he found his patient had but slight pains, and the waters were draining off. The labour having made some progress, he was enabled to determine that the head presented favourably; but a sharp pain succeeded, and the unfortunate woman announced, by the shrillness of her cry, that the uterus was again torn. She fainted, and the pains became weaker. The head of the child was remov-

\* Journal de Med. for 1768.

† Pathologie Chirurgicale, par M. Lassus, tom. ii. p. 237.

ed, and could not be felt even by carrying two fingers some distance in the ruptured uterus. Gastrotomy was quickly performed. The belly of the woman was so tender as not to be touched, and the body of the child did not incline more to one side than the other. It was decided the operation should be performed on the right side, on the presumption that the uterus was ruptured on that side, as on the former occasion. The incision was made within a finger's breadth of the old cicatrice, but a little more external. The peritonæum being opened, it was found that the uterus and parts of the intestines adhered to the cicatrice. Most of these adhesions were carefully destroyed; the hand was introduced into the abdomen, and the feet seized. The child was placed lengthways as regarded itself and mother. The extraction of the child and also of the placenta were made agreeably to the rules of art. The child lived for about half an hour. Mr. Lambron did not discover during the operation the part of the uterus which had suffered the laceration. The intestines which protruded were replaced, and the edges of the wound were approximated by means of sutures sufficiently distant from each other to permit the escape of the extravasated blood. This operation resulted in the complete recovery of the woman, who again became pregnant for the third time, and was delivered naturally in August, 1781, of a healthful child, but of rather a small size."

We perfectly agree with M. Lassus,\* that this operation to be successful, should be performed "as quickly as possible after the accident, while the patient still retains strength; and that the incision should always be made on the side of the abdomen which corresponds with the rupture of the uterus," were this always practicable; but this, even from the history just recited, was not the case, or if it were, it was neglected. For it is not always the sides of the uterus which give way, and consequently when the wound takes place in either the anterior or posterior portion, it is

\* Path. Chirur. tom. ii. p. 239.



probable the fœtus will be found nearly in the middle of the abdomen ; and from our own experience in these cases, we believe there can be but little difficulty in ascertaining to which side it most inclines, by tracing it through the parietes of the abdomen.

Having placed the operations of gastrotomy, and the Cæsarean section, in the only light they should be viewed in, as regards the accident in question, we shall now proceed to point out the mode to be pursued, under the varied divisions we have made of the rupture of the uterus.

### I.

In cases similar to I. of our scale, where *a.* and *c.* are combined, as at 1. or where the pelvis is well formed, but where the uterus contracts firmly, we should proceed to the operation of gastrotomy ; for in this case, the hand cannot be passed through the uterus to deliver per vias naturales. But where I. and *a.* and *d.* are combined, as at 4. or where the uterus remains flaccid, there may be a possibility of delivering per vias naturales, and this may be attempted.

In cases where I. and *b.* and *c.* are combined, as at 3. or where the pelvis is deformed and the uterus contracted, we have no other alternative but gastrotomy. Where I. and *b.* and *d.* are combined, or where the pelvis is faulty and the uterus flaccid, our steps must be regulated by the extent of the deformity—if it be such as will not permit the passage of a child, at full term, we must open the abdomen—but if the deviation be not so great as to prevent this, we may attempt delivery by the first mode.

### II.

In cases such as II. where *a.* and *c.* are combined, as at 1. or where the pelvis is well formed, but the uterus contracted, the only chance is by gastrotomy, and then cutting through the peritonæal coat of the uterus, so as to free the child from it. But where II. and *a.* and *d.* are combined, as at 4. or where the uterus remains flaccid,

we must attempt the delivery by the first mode. And where II. *b.* and *d.* are combined, as at 4. or where the pelvis is deformed, we must proceed as directed, for I. *b. a. d.*

### III.

In cases such as III. where *a.* and *c.* combine, or where the pelvis is well formed, but the uterus, as far as it is concerned, contracts firmly, we should employ the first mode, unless the rent in the vagina be so large as to permit without much greater tearing, the child to pass through the pelvis. In the latter instance, we believe there would be less risk in a moderate extension of the vaginal wound, than from gastrotomy. But where III. *a.* and *d.* are combined, as at 4. or where the pelvis is well formed, but the uterus is inert, we can with great expectation of success, attempt delivery per vias naturales.

In cases III. where *b.* and *c.* are united, as at 3. or where the pelvis is deformed, and the uterus contracting, it is scarcely to be expected we can succeed by the first mode, although the pelvis may not be very faulty; we may, however, attempt it by very gentle means first, but be sure to desist, should a great deal of force be deemed necessary; in the latter case, we can only expect to relieve by the second mode of operating. But where III. *a.* and *b.* and *d.* are existing together, it is possible to deliver per vias naturales, if the deviation in the pelvis be not great; but should it be considerable, our only chance is by the second mode.

### IV.

In cases like IV. where *a.* and *c.* are combined, as at 1. or where the pelvis is well formed, and where the uterus contracts, it will immediately occur that the latter cannot, either in this case, or where the uterus remains flaccid, oppose any difficulty to delivery according to the first mode; so that in these two cases, we cannot believe gastrotomy can be justifiably performed. But were IV.

and *b.* and *c.* or *b.* and *d.* are combined, or where the pelvis is faulty, and the uterus is either contracted or flaccid, it may be indispensable, provided, the deviation in the pelvis be such as would prevent a child at full term to pass, unless the child be very small.\*

## V.

The management of the cases V. where I. and III. are complicated with a descent of intestine, is precisely the same as regards the rupture of the uterus, as directed for these cases, together with a strict attention that the protruded or strangulated gut may be reduced. This is, perhaps, one of the most formidable cases that can present itself to the already but too much perplexed practitioner; for he has not only to contend with a ruptured uterus, but has a strangulated intestine to add to its horrors. We are but too well aware that the latter alone, if not quickly relieved, is sufficient to destroy the patient; and the practitioner may, after flattering himself he has by the delivery of the child and the placenta, given his patient a chance for life, have the mortification to see her expire from this cause, which he only detects after her death. Where this complication takes place, with a contracting uterus, whether the pelvis be well or ill formed, there is no possible chance but in gastrotomy—nor are we certain that even this will give a chance of recovery; for how shall we be led to suspect that a portion of intestine is included in the contracted wound? Does not this difficulty suggest the propriety and authorise the act, to pass the hand freely and carefully round the uterus, with a view to discover whe-

\* Baudelocque says, § 2178, "the section of the covering of the abdomen will not appear so indispensable after a rupture of the vagina, as after that of the uterus," but without specifying the cases in which it might be necessary. He appears to have forgotten what he had just before advanced, that the Cæsarean section (and consequently gastrotomy, when the uterus is ruptured) is exclusively indicated where the pelvis is absolutely too narrow; and adds, "it is much less the fear of a rupture of the uterus, which leads us to perform it, than the impossibility of terminating the delivery in any other way." Is this then not deciding that gastrotomy should be performed in cases of ruptured vagina, when the pelvis is "absolutely too narrow?" Baudelocque, vol. iii. § 2173, p. 424.

ther any part of the gut may be found detained in the lacerated portion of it? For, from merely reasoning on the subject, it would appear very probable that it might be readily detected, as it would prevent the hand from passing uninterruptedly over the surface of the hardened womb. But should it be discovered, how is it to be relieved? That it would be difficult, we readily admit; but, that it is not beyond the resources of a well instructed accoucheur, we are as ready to believe. An incision cautiously made so as to enlarge the opening, would certainly free the intestine from its confinement; but whether the part, by which it is held in durance, would always be sufficiently in view to ensure a successful operation, may reasonably be doubted; though there is a strong probability that this combination would only happen upon either the anterior portion of the uterus, or upon its sides. Now in either of these situations, we should conclude, *à priori*, that it would be possible to liberate the gut.

If the intestine descend through the opening in the uterus, and the latter remain flaccid, and the pelvis well formed, it may be returned after the delivery of the child. But if it take place under similar circumstances with a pelvis too small to deliver *per vias naturales*, it may remain down, after the operation of gastrotomy has been performed for some time, without betraying its situation; nor, indeed, until the returning powers of the uterus should include it within the edges of the wound, and thus strangle it; in this case, the patient may have had the appearance of doing well, and perhaps was doing well; and possibly might have recovered, but for this new accident, for which no remedy could be offered, as we could not know its existence, nor remedy it if we did; for we presume no one could have the hardihood to open the abdomen a second time, upon the bare suspicion that a strangulated gut was the cause of the existing untoward symptoms.

Baudelocque\* mentions a case that was communicated

\* Midwifery, vol. iii. p. 433.



to the Academy of Surgery of Paris by a country surgeon, who declared he had relieved a portion of strangulated gut, by carrying his hand, armed with a bistory, into the uterus, and enlarging the ring formed by the contracted wound, and this, three days after delivery. There is considerable difficulty with us, to reconcile this story with the concomitant facts; for the uterus is stated to have so much contracted the dimensions of the wound, as to impringe upon the protruded gut. Now if the uterus was so reduced as to strangulate a portion of intestine, how was it practicable to carry the hand within it, and find sufficient room there for this display of surgical hardihood and adroitness? We confess ourselves at a loss to imagine it.

## VI.

In such cases as VI. where *a.* and 1. are combined, or where the head is at the superior strait of a well formed pelvis, we should not hesitate to turn or deliver by the forceps, provided the attending symptoms would justify the belief that the uterus was ruptured, and the os uteri sufficiently dilated to admit of either of these operations. We should prefer the former mode, when the waters had been but recently drained off, or when the uterus was flaccid, the latter when the contrary obtained. Where *b.* and 1. existed, or where the pelvis was well formed, but where the head was engaged in the superior strait, the forceps are exclusively indicated, unless we are sure of the death of the child, then the crotchet must be used. And where *c.* and 3. are united, or where in a well formed pelvis, the head is low, with a firmly contracting uterus, the forceps must terminate the delivery, if the child be living—the crotchet, if dead. If *c.* and 4 unite, or where the head is low, the uterus inert, and especially if the child has in great part escaped from it, the forceps alone must be thought of, unless the child is certainly dead; in this case, the crotchet. If any other part than the head present, the case must be treated as if that particular case required immediate manual assistance; for instance, should the

breech present, and not rapidly advance, we should bring down the feet, and deliver, the same if the knees; and should the feet, or but one present, we should deliver as quickly as would be judicious under any other circumstance where it was proper for art to interfere. Where VI. and either *a. b.* or *c.* are united with 2. 3. or 4. the same rules will apply as when this accident happened in a well formed pelvis. If any other part besides the head present in a pelvis but moderately defective, we must conduct the labour with strict regard to the woman's safety and the child's welfare; if it be dead, we may expedite the delivery without regard to the child.

## VII.

In cases like VII. where *a.* is combined with either *b.* or *c.* or where the pelvis is much deformed, so as to preclude the possibility of a child at full term to pass alive; and whether the uterus be contracted or flaccid, there is but one alternative either for mother or child, and that is the Cæsarean section. We are aware that the crotchet may be recommended in these cases, but with what prospect of success? None certainly to the child, for that must necessarily fall a victim; and what is the chance for the mother? We believe none whatever—for the force that would be requisite to open the head, and the extreme difficulty to extract it, if that could be accomplished, would exhaust the woman, and she would most probably expire before the delivery were effected.

With respect to the mode of performing these operations, we have nothing either new or material to offer; we must, therefore, refer to Baudelocque and others for the details on this subject. As regards the after treatment, it must be conducted as if we were treating for peritonæal inflammation, under its various states and conditions of the system, with the addition of a wound in the external teguments; as it is through the medium of this wound, in many cases, we expect the extravasated blood, &c. to issue, we should take care that it should not be closed too soon or too strictly.

OBSERVATIONS ON

**RETROVERSION OF THE UTERUS, &c.**

AS READ BEFORE THE ACADEMY OF MEDICINE,  
18TH DECEMBER, 1820.

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THE disease on which we are about to offer a few observations, has only been accurately known, since the year 1754. At this time the late Dr. Hunter met with a case of death from this cause, which so powerfully excited his interest, that it led to a complete and accurate knowledge of the changes induced upon the uterus, which constitute it. Since this period there is scarcely a writer on midwifery who does not notice it ; yet notwithstanding this familiar mention of the complaint, practitioners are by no means agreed, either as to its importance, or its particular management. Indeed, there is scarcely a disease to which the human body is liable, on which there is such a discordancy of opinion. While one set of practitioners\* view it as an accident of the most serious kind, another regards it as a matter of little concern or importance†—both cannot be right ; but on which opinion shall the young practitioner repose confidence? He will be distracted by discrepancy, and will either negligently wait, and trust to the powers of nature, or will unnecessarily subject his patient, to both pain and hazard.

With a hope that this variance of opinion may without

\* Hunter, Baudelocque, Meygrier, Burns, &c.

† Denman and Merriman.

difficulty be reconciled, the present observations are ventured.

In prosecuting our purpose, we shall, first, give the history of this complaint. Secondly, animadvert upon Dr. Denman's theory and mode of treating it. Thirdly, offer some observations on the mode of reduction, &c.

It is now no longer liable to question, that this complaint can take place, with the unimpregnated uterus, though the impregnated is much the most liable to it, as it necessarily offers a larger surface to be acted upon by the causes which may be capable of producing it.\*

The remote causes are all those, which may tend to depress the fundus of the uterus; and may be external violence, such as blows, pressure, sudden exertion, &c. or they may be violent efforts to vomit, or incessant coughing; an over distended bladder, or perhaps an unusual accumulation of fæces in the rectum or sigmoid flexion of the colon.

When either of these causes act so as to produce the retroversion, it is by carrying the fundus backwards and downwards, so as to place it between the vagina and rectum, by making it fold upon itself—it will follow then as a consequence, that if the fundus be thus depressed, the neck or mouth of the uterus will be elevated, in an opposite direction, and will be found on the anterior and superior part of the pelvis, and immediately almost behind the symphysis pubes. The fundus may have different degrees of depression, and the mouth of elevation—hence some have divided this complaint into complete and incomplete retroversion.† This change in the situation of the uterus may be induced suddenly, or it may occupy considerable time before it becomes established. In three instances we saw it almost instantly produced; in one, by a severe blow across the back while stooping; in the second, by attempting to lift a heavy tub of clothes; in the third, by an effort to jump a

\* Baudelocque, Denman, Merriman, Burns, &c.

† Hunter's Med. Obs. vol. ii. Meygrier, Nouveaux Elémens, p. 116, vol. i.



fence, when pursued by a bull.\* Baudelocque and others furnish us with instances of its gradual but evident approach to retroversion.

The symptoms which accompany this displacement of the uterus are more or less intense, as this viscus may be larger or smaller, or as it may be of recent occurrence, or of long standing. When suddenly induced, the symptoms are, for the most part, violent and alarming. In the cases we have seen, there was a stoppage of the urine and of the fæces; pains resembling those of labour; and a disposition to syncope; nor was there much alteration in these symptoms until the fundus of the uterus was restored. When it is a long time retroverting the symptoms are milder, and only acquire intensity when the displacement has become complete. During the progress, and sometimes even after the change has taken place, the sufferings of the woman are not extreme. We find her complaining of a difficulty in making water, with an increase of desire to do so; a painful dragging pain about the hips, loins, and thighs, and a forcing bearing down pain resembling labour. These symptoms may continue for a considerable time without augmented suffering; but this mild condition of the complaint must necessarily have a limit, should the fundus not spontaneously restore itself; for if the uterus contain an ovum, it will go on to be developed for some time, with nearly as much certainty and rapidity as if this accident had not happened; but it will follow as a consequence that the inconveniences just mentioned will increase in proportion to the augmentation of the uterus, so that there will be an entire stop put to the evacuation of both urine and fæces; this will provoke an intense desire to discharge both the one and the other, which so far from being effective, will but increase the difficulty, by forcing the uterus lower and lower, and thus compressing the neck of the bladder and rectum with still more strictness; this will be accompanied with extreme pain, and if the patient be not speedily

\* Baudelocque, Meygrier, Dr. Evans's Med. Com. vol. vi. p. 215. &c. &c.

relieved, the bladder will either burst,\* or suffer from inflammation and gangrene† and death quickly ensue.

In the unimpregnated state of the uterus, the symptoms, as far as my experience goes, never arrive at this melancholy height; and for this plain reason, that the uterus in such cases, I believe, never acquires so much size as to entirely and intimately occupy the lower portion of the pelvis, and consequently cannot completely obliterate the urinal or fæcal canals. But, although the symptoms which attend this particular condition of the uterus be less severe, it would seem that the parts never become reconciled to this change of situation, however long they may be subjected to it; at least this is the case with a patient now under my care. This lady has at this time all the milder symptoms which accompany this disease, together with considerable leucorrhœa. The uterus has been retroverted most probably ever since her last labour, a period of nearly eight years. From her history of herself, it would seem to have been produced very soon after delivery, for then all the inconveniences which she now suffers began to manifest themselves; nor from that time to the present moment has she experienced any alleviation. Her case I fear to be irremediable, as I apprehend adhesions are formed which will prevent the restoration of the fundus, at least it has hitherto resisted moderate efforts to reduce it.

The most usual period of occurrence in the impregnated uterus is from the second to the fourth month of gestation; for after this period, its volume will be such, as to preclude (*cæteris paribus*) the possibility of its happening. At this time the fundus of the uterus is about to emerge from the inferior strait, and may be felt immediately above the pubes, and has now a kind of resting place offered it by the projection of the sacrum. But that it has happened after this period cannot be questioned; Smellie‡ furnishes us with an instance at the fifth month; and I myself saw a

\* Mrs. Lynn's case. *Med. Obs. and Inq.* vol. v. p. 388. Doeveren as quoted by Merriman.

† Dr. Bell's case, *Med. Facts.* vol. viii. p. 32.

‡ Vol. ii. p. 133.

case in consultation with Dr. Gallaher at between the sixth and seventh months. But in these deviations, we must seek for the cause of the retroversion, either in the unusual size of the pelvis, or the small size of the ovum, from retarded or deficient development. In the patient just mentioned there was retarded development, from her labouring under a confirmed phthisis pulmonalis; her cough was severe; and in a fit of more than ordinary severity and length, the uterus was retroverted; the symptoms from it were violent, but they ceased immediately upon the restoration of the fundus; the patient died about two weeks after. This furnishes an example of retroversion without previous suppression of urine.

This awkward situation of the uterus, does not, however, so derange the economy of gestation, as to prevent the farther increase of the fœtus: \* it will, in its augmentation, occupy such parts as will offer the least resistance to its expansion, and will consequently, if it be left sufficiently long in this situation, gradually fill the whole of the pelvis inferior to the upper strait; when this happens, the consequences are obvious; neither urine nor fœces can be evacuated; and the urethra will be so compressed, as to prevent the introduction of the catheter; and the rectum so obliterated as to refuse the transmission of the most forcibly projected injections. † From this state of things, will result the serious accidents just noticed.

It will be evident then, that should the fœtus continue to be developed for a sufficient time, it will acquire such size as will exceed the opening of the superior strait. Such was the case in the instance which Dr. Hunter ‡ first saw; the symphysis pubes was obliged to be divided before the uterus could be raised, and this also happened in Mr. Wilmer's § patient. From this it would appear that there is a period at which we should attempt the relief of the patient, and beyond which it would be wrong to wait.

\* Baudelocque, &c.

† Ibid.

‡ Med. Obs. and Inq.

§ Wilmer's Cases, p. 144.

But, as all the unpleasant symptoms we have enumerated may proceed from other causes, it is proper, so soon as symptoms become urgent, to determine the nature of the complaint by the touch. If the uterus be retroverted, the finger will, immediately upon its entrance into the vagina, find this canal more or less occupied, more especially on its posterior and inferior part, by a firm roundish tumour, which evidently has an interposing substance between it and the finger, and that substance is the vagina itself; this tumour may be of greater or less size, as it may be the unimpregnated or impregnated uterus, or the degree of advancement of the latter, that may be thrown down; the progress of the finger towards the projection of the sacrum will always be interrupted by this tumour, while it may find a passage immediately behind the symphysis pubes, which will conduct it to the neck or mouth of the uterus; this will be found more or less remote from the inferior edge of the symphysis, as the bladder may be more or less distended, or as the fundus uteri may be more or less depressed in the pelvis. We are told that in some cases the neck of the uterus is removed beyond the reach of the finger; but I have never yet met with such an instance, though I can readily admit the possibility under certain circumstances.

This disease may, however, be confounded with a prolapsus uteri; but it is very easily distinguished from it, by the latter not being behind the vagina; by being readily moveable; by *always* presenting the neck of the uterus in advance at the inferior edge of the os externum; by its never producing the same intensity of symptoms. It may be nevertheless confounded, according to Burns,\* with a diseased ovarium when it may chance to occupy this part, or with an extra uterine conception when it may have descended between the vagina and rectum; but in this I cannot agree with him, for in neither of these cases do we believe that the fundus of the uterus can be carried down with these parts; if this be so, it will be found that the os

\* Principles of Midwifery, p. 155.



uteri will always be within reach, especially after drawing off the water ; it may serve also to distinguish the latter cases from retroversion, by searching for the direction of the uterus by the introduction of a catheter or long probe, as was practised by Mr. White.\* When I say “ as practised by Mr. White,” I only refer to the act, and not to the object he had in view ; with him it was an attempt to pierce the membranes ; and I recommend it, to distinguish between a retroversion of the uterus, and the descent of an ovarial tumour, or an extra-uterine ovum. It was found in a case of most probably a diseased ovarium, that a probe could be introduced into the os uteri, which decided the direction of the body and fundus, and which was found to be “ obliquely upwards and backwards ;” now had it been a genuine retroversion, the direction would have been backwards and immediately downwards. And this method may be practised in cases of extra uterine pregnancy, with perhaps even more certainty of success ; for in a case of this kind related by Dr. Kelly,† it was found on dissection that instead of the ovum carrying the uterus downwards, it was found raised up in the pelvis by the head of the child, and “ had the usual appearance of a womb in the unimpregnated state.” Now, in a case like this, there could have been no difficulty in passing a catheter into the uterus, which at once, from its direction, would decide it was not retroverted.

From what I have seen of this disease, and from attentively considering a number of the cases upon record, I would propose, that this method should be adopted, to determine the nature of the tumour found behind the vagina, and which leads to the ambiguity just mentioned. It is unquestionably highly important to determine the point in question ; and more especially, early after the untoward symptoms manifest themselves—for if this be neglected in retroversion, it may put it out of our power to profit by the knowledge ; and if it be either an ovarial tumour or an ex-

\* Med. Com. vol. xxvi. p. 435.

† Med. Obs. and Inq. vol. iii. p. 44.

tra uterine ovum, it may lead to the employment of means that may relieve the suffering patient.

A confidence is felt, that the method just proposed would dissipate all doubt upon the subject, and lead us to that kind of certainty as to the nature of the disease, as would at once decide the mode that should be resorted to for relief. As far as I have been able to determine by examining the histories of ovarial and extra uterine tumours, there has not occurred an instance of the fundus of the uterus being depressed by them; consequently, determining the direction of the uterine cavity in doubtful cases as proposed, will prevent error in the means of cure. An interesting case is related by Giffard,\* of an extra uterine *fœtus* being deposited between the vagina and rectum, giving rise to all the symptoms of retroversion, and from which part it was eventually extracted; the mother however died; upon dissection, the uterus was found "driven upwards and forwards by the *sacculus*" which contained the *fœtus*; and it was observed "that there was a fulness and hardness very perceptible to be felt outwardly in the fore part of the belly, some distance below the navel," and this was found to be the uterus. We may then, perhaps, in these cases, be always able to feel the uterus immediately over the pubes, and this may serve an important purpose as regards diagnosis—but this is purely conjectural—yet it may deserve attention.

We may also observe, that in cases of extra uterine ova, or ovarial tumours, their progress is slow and pretty regular—the latter especially is almost always very slow, consequently the symptoms which might confound them with retroversion, must be very gradual in their approach, and must occupy a long time before they create serious difficulty—the history of the case then, may furnish us with important facts.

Having thus briefly given the history of the retroversion of the uterus, we shall now offer a few observations on Dr.

\* Midwifery, p. 335.

Denman's theory and mode of treating it. He says, "There is in every case a suppression of urine, with extreme pain, and by its continuance such distention of the bladder, that the tumour formed by it in the *abdomen* often equals in size, and resembles in shape, the *uterus* in the sixth or seventh month of pregnancy. But it is necessary to observe, that the suppression of urine is frequently absolute only before the retroversion of the *uterus*, or during the time it is retroverting; for, when the retroversion is completed, there is often a discharge of some urine, so as to prevent an increase of the distention of the bladder, though not in sufficient quantity to remove it."\*

In this account of the retroversion, it will, I trust, be made to appear, that Dr. D. has forced fact to comport with his theory of the production of this disease; I shall therefore first state his opinion upon this subject, that our observations may be the better understood. He is of opinion that the suppression of urine always precedes the retroversion, and that it is the *cause*, and not the *consequence* of this affection. And for the uterus to be retroverted, it is necessary it should be elevated in the pelvis, and that this is effected by a distension of the bladder from an accumulation of urine.† Now it is essential to this theory, that a suppression of urine should exist before the retroversion takes place, and that the stoppage should only be "absolute" then "or during the time it is retroverting." We would inquire what gave rise to the suppression of the urine, which is so essential to the retroversion? Not a word is said in explanation of this question. Does it really exist at the time Dr. D. supposes, so as always to produce the disease in question? We think we have, from our own experience, as well as that of others, strong reason to doubt it.‡—First. Because no one, with the exception of Dr. Merriman, has corroborated his opinion by

\* Introduction, p. 137.

† Ibid, p. 139.

‡ We do not mean to deny that a distension of the bladder from an accumulation of urine may produce a retroversion, but only to insist that it is, and has been frequently produced without it.

adopting his explanation.—Secondly. Baudelocque declares, that in a certain Marchioness the retroversion took place instantly, and “from that moment she found it impossible to evacuate a *single drop* of water.” And this is conformable to my own experience in at least four cases. —Thirdly. Baudelocque tells us he demonstrated a slow inversion\* of the uterus to his pupils, and that the inversion was not complete till after three or four weeks. He does not mention any difficulty in making water, nor a suppression of it, which, agreeably to Dr. D. must be absolute “before the retroversion, or during the time it is retroverting.” Now, had either of these circumstances obtained, it would, from the known accuracy of Baudelocque, have been mentioned; but, on the contrary, he observes, that it was not until the inversion was complete “that the woman found herself obliged to submit to the necessity of reducing it.”

Dr. D. declares “the uterus must be elevated before it can be retroverted.”† To disprove this, it is only necessary to recur to the cases we have already mentioned, where it was instantaneously induced; and where it is impossible to conceive that the uterus could be elevated. Notwithstanding this, we would agree with him, that it could be more easily retroverted when this condition exists. We mean then merely to insist, that elevation is not a *sine qua non* to retroversion, and thus disprove a part of Dr. Denman’s theory.

But if we admit Dr. D’s diagnosis of this disease, we shall perhaps be forced also to admit the justness of his general positions; we shall therefore immediately advert to them. He says, “If a woman, about the third month of pregnancy, has a suppression of urine continuing a *certain* length of time, and producing a *certain* degree of distension of the bladder, we may be assured that the uterus is retroverted.”‡ This is a notable instance of the want of

\* The term inversion is used indiscriminately with retroversion in Heath’s translation of Baudelocque.

† Introduction, p. 139.

‡ Ibid.



precision; for we cannot possibly determine the cause from the consequence; for the employment of these conveniently ambiguous words, "*a certain time*," and "*a certain degree*," leaves to us the choice of either. If a suppression is so complete as to produce "a certain degree of distension," then we may be assured "that the uterus is retroverted," provided the woman is three months advanced in her pregnancy. And why are we assured that this is the case? Is it because a suppression of urine will always produce a retroversion, or because a retroversion will always occasion a suppression of urine? Now the first, we know, is not strictly the case, as we have many times known a suppression of urine without a retroversion; but we have never known the converse of this. Or shall we suppose Dr. D. to mean, that if a "certain degree" of distention of bladder be continued a "certain time," that is, long enough to produce a retroversion, "we may be assured" there is a retroversion existing? Can we be surprised that Dr. D. regards "retroversion" of so little consequence either as to its mediate or immediate consequences, when he establishes the disease upon such a diagnostic as that "if a woman complain for a certain time of a suppression of urine, we may be assured that the uterus is retroverted?" If this constitutes the disease, he may well be justified in leaving it to nature.

Dr. D.\* says a little further, "But the preceding suppression of urine may be overlooked, as there is not occasion for it to be of long continuance in order to produce its effects, especially in a woman who hath a capacious pelvis, in whom the retroversion of the uterus is most likely to happen." Can we imagine that so distressing a circumstance as a suppression of urine could possibly be overlooked, though there should "not be occasion for it to be of long continuance in order to produce its effect?" Let any one who has experienced this inconvenience for even a few hours, be asked whether he remembers ever to have

\* Introduction, p. 140.

thus suffered, and his answer will speedily convince us it is not easy to be "overlooked." We cannot then believe it would be so with a woman, merely because she has a "capacious pelvis:" for a pelvis of any capacity cannot relieve the pain arising from a suppression of urine, though this be "not necessarily of long continuance." If the pelvis be very large, it may give rise to a retroversion at a more advanced period of gestation than a smaller one; and this is all that it can do: for if we understand the import of a "capacious pelvis," it means one rather beyond the ordinary standard; and if this be true, we deny any greater facility to retroversion, in such a one, than in one of the ordinary measurement at the period fixed for this accident by Dr. D. himself: for at the third month there is abundant room for retroversion in a pelvis of ordinary size. We might indeed with much plausibility contend, that a "capacious pelvis" is less favourable to retroversion, by employing the fact, that in such pelvis the uterus is habitually lower than in common sized ones.

Dr. D.\* observes, "It has been said that the state of retroversion was injurious to the uterus itself, and would produce some dangerous disease in the part: it has also been asserted, that if the uterus was permitted to remain in that state, it would be locked in the pelvis by the gradual enlargement of the ovum, in such a manner as to render the reposition impracticable, and the death of the patient inevitable." He contends, that† "for both these consequences there cannot surely be reason to fear; for if the uterus be injured there will be no further growth of the ovum; and if the ovum should continue to grow, it is the most infallible proof that the uterus has not received any injury."

From this statement a young practitioner would irresistibly be led to the conclusion, that no evil would arise even from an unreduced uterus, than which nothing can be further from the truth. We shall, we trust, prove

\* Introduction, p. 143.

† Ibid. p. 143 and 4.

that neither member of this dilemma is founded on fact or upon correct observation. We will, therefore, examine them a little more at large.

Dr. Denman tells us, that, "If the uterus be injured, there will be no further growth of the ovum"—from this it would be natural to conclude, that if the growth of the ovum were interrupted, all the evils that could arise under the circumstance of retroversion would be at an end, and that consequently it would be desirable that this should happen, since agreeably to this it would effectually put a stop to the mischief that would ensue if it were progressive in its development. It is true that the meaning we have given to this passage is by implication, but we sincerely think the premises warrant it—for what are we to conclude from it, if it bear not this construction? If the uterus be injured by the retroversion, it would seem to be premised, that this injury would be repaired by the ovum ceasing to increase. Now to us it appears that two positive evils would arise from this condition of the uterus—the one, the mischief that is done to the uterus itself; and this mischief we cannot limit at pleasure, for if it be so great as to cause the death of the ovum, it may also be so great as to cause the death of the patient. The second is the destruction of the ovum; which, when it happens, must sooner or later eventuate in abortion; and the consequences of abortion while the uterus is retroverted, are sometimes very serious indeed.

If risk is to be incurred from the uterus throwing off its contents, let it be from pursuing a plan that offers at least some probable chance of relief to the patient—namely, the attempt at reduction—for should we succeed, abortion may not follow, for (as we shall remark more at large presently) it is not a necessary consequence; but should it take place after the restoration, the sufferings of the woman will be much diminished by the natural position of the uterus being restored; and should we fail in our attempt at reposition, the patient will be in no worse situation than before.

In confirmation of this, we shall relate an interesting case given by Dr. Bell.\* We shall take the liberty however of abridging it, but shall not suppress any material point. A woman in the 36th year of her age and mother of nine children, was attacked with great pain and difficulty in making water, for which Dr. Bell prescribed several remedies without seeing of her. She however became worse, and he was requested to visit her, which he did accordingly. He found her in much distress and pain about the abdomen, &c. with frequent vomiting and obstinate constipation. Upon examination per vaginam it was found that she was labouring under a retroverted uterus—Every thing was done that the necessity of the case seemed to suggest, but without the smallest benefit, as the attempts at reduction were unavailing.—Symptoms of labour soon supervened, and after a short continuance, the fœtus was expelled, and the patient died a few hours after. Leave was obtained to inspect the body, and the following is the account of the dissection in Dr. Bell's own words. "On turning up the intestines, the bladder was discovered flaccid, and much enlarged. Here the inflammation had made great progress. The peritonæum lining [covering] the bladder, and for a considerable distance round it, was covered with a strong, thick, inflammatory crust. The coats of the bladder were every where much thickened, and upon the posterior and superior part of it, there was a portion of the size of a half crown, in a state of mortification."—"There was very little urine in the bladder. The uterus was found to have regained its natural situation in the pelvis. This organ appeared not to have suffered from inflammation." Now the death of this patient could only be attributed to the injury arising from retroversion.

Let us now examine the second member of Dr. D's dilemma.—"Should the ovum continue to grow, it is the most infallible proof that the uterus has not received any material injury." Consequently we are to conclude, that

\* Med. Facts, vol. viii. p. 32.



no future mischief can arise. But does this rest upon that foundation on which it should alone rest, experience?—Certainly it does not. The uterine ovum goes on to be developed as we have already noticed, and eventually, if not interrupted, will fill up the whole of the inferior strait—its volume becomes too great to pass through the superior strait; it will completely obliterate the rectum or allow none but the very thinnest *fæces* to pass; the urethra, or neck of the bladder, will be so compressed as to interrupt the flow of urine; the consequence is, that the bladder will suffer, as has already been noticed, and most probably *death* will be the result.

It is a matter of no consequence in the discussion of this subject, which organ of the pelvis suffers so as to produce death—it is the uterus being in a state of retroversion that creates the mischief, and this is all that is necessary to contend for.

But has Dr. D. or any body else, seen an instance in which the uterus continued to develop itself, which was not ultimately attended with serious distress or perhaps death? *We* know of no such instance upon record.

When a gentleman sets out determined to attach his own valuation to the inconveniences and dangers to which a patient may be exposed, without the smallest regard to the value the sufferer may fix to them, he may easily very much underrate them, and without difficulty be betrayed into the following assertion, “that the uterus may remain in a retroverted state for many days or weeks, without any other detriment than what may be occasioned by the temporary interruption of the discharges by stool and urine.”\* Now, we would inquire what inconveniences can well be considered greater, than a suppression of urine, or in an incapacity to pass *fæces*? Dr. D’s attempt to modify this unpleasant, and often hazardous condition, by employing the word “temporary,” will not alter the nature of the sufferings of the patient, nor diminish them, either

\* Introduction, p. 145.

in point of duration or intensity—and she will not consider the endurance of pain “for many days or weeks” as “temporary.” But it may be said, that the word “temporary” will not convey the idea of such extended suffering; perhaps at first sight it will not, nor was it intended it should; but let us recur to the fact about which there can be no dispute, that the inconveniences stated are always as permanent as the influence of the cause which produces them; and consequently, that if this continue, “the interruption to the discharges by stool and urine” will continue; therefore, “temporary” in this instance must mean a period commensurate with the duration of the cause.

Dr. D.\* again observes, “that the enlargement of the uterus, from the increase of the ovum, is so far from obstructing the ascent of the fundus, that it contributes to promote the effect, the distention of the cervix becoming a balance to counteract the depression of the fundus; for I have found no cases of the retroverted uterus admit of a reposition with such difficulty as in women who were not pregnant, in whom the uterus underwent no change.” Baudelocque tells us, “though we meet with few obstacles to this reduction when the displacement is recent, and the volume of the uterus still small, they are very great, and sometimes insurmountable, when it has existed several days or weeks.” The experience of Baudelocque was at least equal to that of Dr. D. Here we are led to conclude, that so far from the progressive development of the uterus being an injury, it must be regarded as a positive benefit; and were Dr. D’s statement strictly correct, it would be so—that is, if the “cervix” should augment so much that its weight would exceed that of the fundus, and thus from its gravity restore it. But this is far from being the case, for there cannot be the smallest advantage derived from the “distension of the cervix” as an antagonising power to the fundus; for the latter will acquire weight in a precise ratio with the cervix, and they will consequently be

\* Introduction, p. 145.

equal to each other. Indeed we may go farther, and consider it a positive evil; for both reason and experience support us in the opinion, that the accidents attendant upon retroversion are augmented in violence by the increased size of the ovum. In proof of these we need only refer to the situation of the "cervix" as regards the bladder; and to the history of those cases, in which this latter organ has materially suffered. With respect to the first, it will easily be understood how an increased size of the "cervix" shall still more incommode the bladder, by pressing upon its neck with augmented force; and as regards the second, it will be found that the bladder has never materially suffered but where there was an increased bulk of the uterus. Now if this be true, and we sincerely believe it to be so, where is the advantage proposed by "the enlargement of the uterus?" Nor does Dr. D's opinion derive the smallest support from the instances which he adduces for this purpose; namely, that empty uteri are more troublesome to reduce than impregnated ones; for after carrying up the fundus of an impregnated uterus above the projection of the sacrum, its bulk is an advantage, for by its occupying a larger space in the superior strait, it is less liable to relapse into its old situation; besides, its elasticity aids its restoration, for being bent down upon itself, something like a retort, it is constantly disposed to restore itself wherever freed from restraint, unless indeed it has so long submitted to this situation that its resiliency is entirely destroyed. The unimpregnated uterus has not this advantage, for it is not so much bent, nor does it occupy so much space; being then more at liberty, it has a stronger tendency to fall back, as from its size, it cannot rise above the projection of the sacrum; it is not, then, as far as my experience goes, more difficult to restore the fundus, but it is confessedly more difficult to retain it in its situation, after reduction.

The following declaration of Dr. D. does not coincide with my own experience, nor is it corroborated by the observations of others. "If the attempt to replace the uterus

be instantly made after the urine is discharged, so much force will often be required for the purpose as will, notwithstanding all precaution, give much pain, induce the hazard of injuring the uterus, and often occasion abortion.”\* If this sentence be literally taken, it would seem to inculcate, that the moment after the discharge of urine is an improper instant to attempt the reduction; yet it is directed by all writers and practitioners to draw off the water, whenever practicable, before we make an effort at reduction; and Dr. D. himself must be understood to give the same advice, when he says “that all attempts to restore the uterus to its natural position, before the distension of the bladder is removed, must be fruitless, as the uterus will be irresistibly borne down by the pressure of the superincumbent bladder. The first step then to be taken for the relief of the patient is to draw off the water.”† Yet he tells us, as just stated, that “if the attempt to replace the uterus be instantly made after the urine is discharged,” we run the risk of provoking abortion. How would a young practitioner act under this contradictory advice?

But we will not insist further on this inconsistency. Let us now inquire whether the apprehensions of Dr. D. with respect to abortion often following the attempt at reduction be well founded. I have never seen it follow in the cases which have fallen under my notice in any one instance. Baudelocque‡ tells us, “that abortion is not always the consequence of such efforts,” and declares he could cite more than twenty instances in which it did not follow. Dr. Hunter tells us, “I have known several cases of the same kind,” (that is of retroversion,) “but in a less advanced state,” (than the one attended by Mr. Wall and himself.) “They all happened about the third month, sooner or later.” “They were all *successfully* treated after the following manner;” and then proceeds to describe his mode of restoring the fundus. But suppose abortion should take place, it does not necessarily follow that our

\* Introduction, p. 146.

† Ibid. p. 141.

‡ System of Midwifery, vol. i. p. 176.



exertions have provoked it; for in the case mentioned by Smellie, this happened after the uterus had spontaneously replaced itself; and it is considered by almost every other practitioner than Dr. D. to be an imperious indication, to restore the fundus of the uterus, nor do they regard the *contingency* of abortion but of minor consequence. Meygrier\* tells us, “Il ne faut pas craindre de provoquer l'avortement par des tentatives réitérés; elles sont bien moins dangereuses pour la mère et l'enfant que la non réduction de la matrice.” Dr. Hunter was of the same opinion.† But Dr. D.‡ confesses that the fundus may be restored in some cases “without much force,” but that in others every attempt has failed. To what is this failure in general to be attributed? We believe to the advice of Dr. D. himself, to treat the complaint as one of little consequence, and to temporise until the uterus becomes so enlarged and so inveterately fixed, as to render every attempt at reduction unavailing. I think we may with much safety conclude, that the alternative offered for reduction by Dr. D. is much more inconvenient, if not hazardous, to the patient, than well directed efforts to restore the fundus. The repeated and daily use of the catheter is not only very troublesome, agreeably to Dr. D's own confession,§ but in many instances so highly repugnant to delicacy, that few would accept of it, to spare themselves the risk attendant upon reposition. Besides it may be but of temporary utility; for no one can promise absolute success from it, and the time to attempt reposition may thus be allowed to pass. Unavailing and painful attempts are then made, and the patient doomed to three-fold sufferings. All the evils which could attend an early attempt now supervene with aggravated force; and the patient, after long and terrible conflicts, at last succumbs.

It cannot be too strongly insisted upon that the patient should be carefully watched, lest the period may pass at which manual assistance may be successful. We may, with-

\* Nouveaux Elémens, vol. p. 122.

† Medical Observations, vol. iv. p. 406.

‡ Introduction, vol. i. p. 146.

§ Ibid. vol. i. 147.

out running much risk, temporise for a time ; but let not that time be exceeded. We are of opinion it should go very little beyond the fourth month : for after this period, the bulk of the uterus may be found greater than the opening of the superior strait. But should alarming symptoms supervene at any time previously, they must most promptly be attended to. Were this rule strictly adhered to, we believe we should seldom hear of a fatal issue to this disease. When death has occurred, it has almost always been from neglecting the proper period for reposition. This was the case with Dr. Hunter and Mr. Wall's patient.\* It was so in two other instances mentioned by Dr. Hunter ;† also in the case related by Dr. Bell ;‡ so also in Dr. Orr's§ patient.

But, concludes Dr. D.|| “ we may bring the matter to this issue : if the uterus, when retroverted, can be replaced by art, without the exertion of much force, or the risk of mischief, the immediate reposition, *though not absolutely necessary, is at all times an event to be wished*; as farther apprehension and trouble are prevented, *the safety of the patient ensured*, and her mind quieted. But (continues the Dr.) when the uterus cannot be replaced without violence, it seems more justifiable to wait for its return, and to satisfy ourselves with watching and relieving the inconveniences produced by the retroversion.” The paragraph we have just quoted is followed by this remarkable observation, “ we shall also find, that the longer the attempt to replace the uterus be delayed, the more easy the operation will ultimately be, and the success more certain.” In one place we are taught to believe that “ immediate reposition” is desirable, as the safety of the patient is thereby “ insured ;” in another, we are advised to withhold this prompt assistance, “ for the longer it is delayed the easier and more certain will be the operation.” We must now ask, why is the immediate reposition at “ all times to be wished,” if the

\* Med. Obs. and Inq. vol. iv. p. 401. † Ibid. p. 409.

‡ Med. Facts. vol. viii. p. 32.

§ New England Journal, vol. ii. p. 133.

|| Introduction, p. 148.

longer we delay should render the operation both easier and more certain? What would be the impression of an inexperienced practitioner who may have resorted to Dr. D. for information? How could he decide on which of these contradictory opinions he should rely?

From what has been said, I think we may fairly conclude from D's own statements, that the woman labouring under retroversion does run a risk of a serious kind by the long continuance of the disease; and that risk so great as to threaten, and sometimes to end in the loss of life. We agree with Dr. D. that this danger does not arise always, nay perhaps not very often, from the injury which the uterus itself sustains, but from the injury inflicted upon the bladder from long continued pressure, and distention. But it is a matter of very little moment which of these organs suffer, provided one or the other will suffer, from the derangement we are speaking of. The bladder, from its peculiar structure, can suffer but a certain degree of distention, without experiencing serious mischief; and if the distraction of its sides be forced beyond this, it may become inflamed,\* gangrenous,† or burst‡—now, as the uterus when retroverted always presses the bladder with more or less force, it will necessarily suffer in proportion to that compression§—if this be to the extent of entirely obliterating the urethra, so that a catheter cannot pass, the consequences just mentioned must ensue. We do consider then, so long as the impregnated uterus be in a state of retroversion, there is no safety for the bladder, and consequently none for the woman. We shall here close the second part of our inquiry.

We shall now proceed to make a few observations upon the mode of reduction, and attempt to reconcile the discordant opinions that are entertained in regard to retroversion of the uterus.

\* Dr. Bell, *Med. Facts*, vol. iii. p. 32.

† Mr. Lynn, *Med. Obs. and Inq.* vol. v. p. 388.

‡ Dr. Squire, *Med. Review* for 1801.

§ Doeveren's case, as quoted by Merriman, p. 13. *Essay on Retroversion*.

From a careful investigation of the cases upon record, and from our own personal experience, we are convinced that Dr. D. has done no inconsiderable mischief, by treating this complaint as one scarcely meriting consideration. It has diverted the public mind in many instances from that attention to the situation of the female while labouring under the disease, to which it is so justly entitled, and thereby permitted the time to pass by, at which assistance would have been available; and death has been but too often the melancholy consequence. That the disease is no longer so formidable as formerly, we grant; but this is not owing to any change in the nature of the complaint, but to its being better understood, and to its being more promptly attended to. On the continent of Europe this disease is constantly regarded with a suspicious eye, and is rarely permitted to produce fatal consequences. They there universally look upon it as a disease of danger, and treat it accordingly; the effect of this is, they have much fewer victims.

It is a fact, as remarkable as important, that almost all the fatal histories of retroversion upon record are to be found in the British publications; to what can this be owing, but to the difference of pathological views as entertained on the continent of Europe, and in the British isles? We do not wish to deny, that Dr D. has great merit in having so successfully investigated the mechanism of this accident, but at the same time must believe that it led him to regard it, as not only of easy production, but of easy cure. In proof of this we need only repeat his diagnosis, that "if a woman, about the third month of pregnancy, has a suppression of urine continuing for a certain time, and producing a certain degree of distention of the bladder, we may be assured that the uterus is retroverted." If Dr D. has uniformly determined the presence of retroversion from these symptoms, without an examination per vaginam, we cannot be surprised at the little consequence he attaches to it, or at the many and wonderful cures performed by nature or the mere introduction of the catheter. The symp-



toms just adverted to, should always put us upon our guard; for the uterus may be retroverted, and it should prompt us to an examination, to put it beyond doubt. But they never justify us to declare this to be the case, without this examination; for I have many times known this difficulty of making water in pregnant women relieved, even without the catheter by the mere exhibition of the sweet spirits of nitre and a little laudanum; and I have in several instances examined *per vaginam*, when it was more than ordinarily severe, without finding a retroversion. I am, therefore, entirely convinced, from an extensive experience, that a suppression of urine may take place at the period designated by Dr. D. and that "a certain degree of distention of the bladder" may be produced (for I have been obliged occasionally to use the catheter) without inducing retroversion. Many have therefore been misled, by believing the partial symptoms assigned by Dr. D. to constitute the disease; these symptoms are of frequent occurrence in pregnant women, while retroversion is comparatively rare.

But let us agree with Dr. D. as to the cause of the disease; does a knowledge of it, render it either lighter or of less moment? Certainly not—nor will the removal of the urine alone, in one case out of ten, be sufficient for the removal of the disease, although it becomes indispensable to the prevention of serious mischief, to the mechanical restoration of the fundus; and a reliance upon it has but in too many instances proved fatal or seriously mischievous to the patient—for we must again insist, that the bare removal of a distended bladder is not alone sufficient. Now, the case ought fairly to stand thus: if the plan proposed by Dr. D. does not always succeed, and if, when it does not, the patient by the increase of the ovum is always seriously deranged by it, and her life not unfrequently lost; and if upon the plan we shall presently propose, no such inconveniences shall accrue, but on the contrary the most decided benefits result, ought we for a moment to hesitate between the two?

It appears then certain, that by temporising we incur risk almost always, and invariably an increase of difficulty in attempting restoration—does it not appear a deduction not to be resisted, that we should always attempt the latter when the former should not pretty speedily succeed? The time we may perhaps safely indulge in, we have already pointed out.

What are the objections to the attempt at restoration? 1st. Provoking abortion by giving pain or doing mischief to the uterus—2. By its being unsuccessful, after having made strong and repeated efforts to this end. With respect to the first, we have already said enough to convince any unprejudiced mind, that it should not constitute an objection. And as regards the second, it has only arisen in consequence of the proper period being permitted to pass, before the operation was attempted, or this not conducted after a proper manner. We have already said enough as respects procrastination, and shall therefore not repeat it; but we will spend a little time in the consideration of the proper mode of conducting the operation.

The usual directions for this are known to every body; it is therefore not necessary to transcribe them. We shall only consider what the forces are which oppose the restoration of the fundus in our attempts to this end, and then point out what we think the best mode of overcoming them. First, a distended bladder. Second, an expanded rectum, and perhaps a loaded colon at its great flexure. Third, the counteracting efforts of the patient herself. Fourth, the too great bulk of the uterus.

The mode of overcoming the first difficulty will immediately suggest itself; but however obvious this may be, it is by no means of always easy accomplishment. It requires both care and dexterity. Dr. Denman's\* observations on this subject are so judicious, that I feel I cannot do better than transcribe them. "The first step to be taken for the relief of the patient is to draw off the water; yet there is

\* Introduction, p. 137.

always a great difficulty in the introduction of the common catheter, because the urethra is elongated, altered in its direction, and pressed against the ossa pubis by the tumour formed by the retroverted uterus ; and many women, when the uterus was retroverted, have lost their lives from want of expertness in introducing the catheter. But the attending inconveniences may be avoided or surmounted by the use of the flexible male catheter slowly conducted through the urethra. I say slowly," continues the Doctor, "because whatever catheter is used, the success of the operation, and the ease and safety of the patient, very much depend upon this circumstance ; for if we attempt to perform it with haste and dexterity, or strive to overcome the difficulty by force, we shall be foiled in the attempt, or it will be scarcely possible to avoid doing injury to the parts. The catheter should not be carried further into the bladder, when the urine begins to flow, unless it ceases before the distension be removed, which in some cases happens in such a manner as to give us the idea of a bladder divided into two cavities. External pressure upon the abdomen, when the catheter is introduced, will also favour the discharge of the urine."

For the removal of the second difficulty, it must be immediately attempted to throw up injections ; but this sometimes is equally as perplexing as the introduction of the catheter ; but the point should not be given up without the thing is really impracticable. In this case we must do without it. But from a case having lately occurred to me, in which the rectum was very strongly compressed by the uterus, though not in a state of retroversion, in which I succeeded in having injections thrown up, by employing a large male gum-elastic catheter for the canula ;\* I am induced to believe, that by a little careful and skilful management it might succeed in cases of retroversion. It should at all events be tried before the thing is abandoned as im-

\* I am originally indebted to my much respected friend, Dr. Physick, for this improvement in the administration of injections.

practicable. The injection should consist simply of salt and water in the proportion of a table spoonful to a pint of water. We may also, a few hours before the attempt at reduction be made, give the sulphate of magnesia in small doses, which has sometimes had a good effect, provided the stomach be free from nausea or vomiting.

The third obstacle we have to contend with, is the violent efforts that are induced to bear down; thus counter-acting all our endeavours at reposition. This is perfectly involuntary, and is decidedly the greatest hindrance we have to contend with in ordinary cases; indeed it renders the most simple form of this complaint, a case of some difficulty, and I have known it the only one to oppose us. In cases where the urine can be drawn off without difficulty, and the contents of the rectum discharged without much trouble, we may meet with such opposition from these involuntary efforts as either to foil us, or to render the reposition extremely difficult. Indeed, as far as my experience justifies the remark, it is almost the only one I have ever met with. Nor do I stand alone in this remark; it must have happened with every practitioner, though not distinctly stated, as far as my recollection serves me, by any but Dr. Bell, who tells us "the pressure (employed in the reduction) however, excited strong efforts of bearing down, which were a considerable bar to the success of the operation; and at one time, when the uterus appeared to be giving way, and I had great hopes of success, the patient, by an involuntary forcible effort of this kind, destroyed all the advantages I had gained."

From the consideration of this opposition, rendering every effort at reduction unavailing, I was tempted to destroy it by inducing a state of faint by bleeding, before I attempted the reposition. This succeeded, and has done so in every instance in which I have tried it. I therefore recommend it with confidence, convinced that in some instances it is the only means we have in our power to do away with this kind of difficulty. It may not always be necessary to employ bleeding, perhaps, to induce this disposition to



syncope ; it may be done by other means, probably with equal advantage, such as tartar emetic by the mouth, or the infusion of tobacco by the rectum ; but of neither have I had any experience.

But before we bleed *ad deliquiam*, we should have every thing arranged for the operation of reposition, that no delay may be created after this state is induced ; we should have previously evacuated the rectum and have emptied the bladder ; the bed should be prepared in such a manner as will allow the patient to lie upon her back, with the perinæum free over the edge of the bedstead, and her shoulders depressed lower than the hips ; care should be taken to have a matrass or some other firm and protecting substance between the back of the woman and the bedstead—the parts should be well lubricated—a chair should be placed for each foot, and the legs must be supported by an attendant for each. When every thing is thus prepared, the patient should be placed on her feet near the part of the bed prepared for her reception, her arm tied up, and from a large orifice subtract as much blood as will produce the desired state of faintness ; so soon as this is observed, the arm should be tied up, and the patient placed as directed above ; the hand, well lubricated, should then be gradually introduced into the vagina in a state of supination ; the fingers retracted in such a manner as to make them form a straight line at their extremities ; these must now be gently pressed against the most depending and and posterior portion of the tumour that is found within the vagina, and carry it backwards and upwards along the hollow of the sacrum, until the mass shall reach above the projection of this bone ; when arrived here, we may withdraw the hand, introduce a sufficiently large pessary, and direct the woman to remain quietly in bed, after having been placed there, for three or four days. We should, for this period at least, draw off the urine by the catheter at least twice in twenty-four hours ; and the bowels emptied by mild injections. This plan has succeeded, where I am persuaded I should have failed without it.

The last difficulty we have to encounter is where the size of the uterus is larger or as large as the opening of the superior strait. This will immediately present itself as one of immense consequence and difficulty—the lives of both mother and child are at stake—but perhaps one not beyond the resources of our art. I am disposed to believe that, where the size of the uterus is not larger than the opening of the superior strait, that we may sometimes succeed by employing the plan just suggested. It should at all events be tried before severer measures are resorted to. If it fail, we certainly do not lose by having made the trial. But what is to be done should it fail? This is a question of great moment, and may be resolved by adopting one of three modes of practice.

First. To confide entirely in the resources of nature, as recommended by Merriman.

Second. To attempt the production of abortion, by breaking the membranes through the os tinæ.

Third. To puncture the uterus through the rectum or vagina, with the same intention, as recommended by Dr. Hunter.

As to the first, we have, from all we can learn, but little temptation to adopt it; and I am of opinion it never should be but as a dernier resource. The second, if practicable, would unquestionably be the mildest and safest; but we are led to believe that it will not always, nor indeed perhaps ever, be practicable; but it should first be essayed, before we have recourse to the third. I taught this to my pupils four and twenty years ago, and I thought until within a few days it was an original suggestion; but in reading the case related by Mr. White, I find it was attempted by him at the request of Mr. Hamilton, afterwards professor Hamilton, under similar circumstances, but without success. It has lately been attempted by M. Jourel,\* who says, in his journal of a very interesting case, which we shall in part relate, “*Le soir du même jour, tentative infructueuse pour*

\* Dictionnaire des Sciences Medicales, art. Deviation, vol. ix. p. 81.

introduire un cathéter par l'orifice de la matrice afin de crever les membranes et de donner issue aux eaux de l'amnios : le col de l'organe, trop fortement courbé, s'opposa à cette manœuvre dont nul auteur n'avait parlé." It would seem that M. Jourel had not met with the case of Mr. White, wherein this operation is recommended, nor the reviewer of his case, who agrees to call it an unprecedented manœuvre. I am however still of opinion, that a flexible catheter might perhaps succeed, if cautiously conducted with this view.

The third alternative, has been universally condemned by the British writers, but it would seem, without sufficient ground ; for it has actually succeeded in the hands of M. Jourel,\* in a recent trial in a case of retroversion. As this case is highly interesting and but little known, I shall take the liberty of condensing and relating it.

" A woman aged 23 years, was at the period of six weeks of pregnancy rudely handled in a frolic ; she was soon after seized with an hæmorrhage from the uterus, accompanied with pains in the back and groins, with a sensation of weight in the perinæum, a difficulty in walking and voiding her fæces. M. Jourel was at the end of a fortnight consulted ; he recommended rest, and the use of some astringent drinks. The woman went into the country, and was not seen for a month by M. Jourel. At the end of this time she told him that the discharge from the vagina had ceased within two days ; but that all her other symptoms had increased, so that she voided with great difficulty both urine and fæces. Relief was attempted by the catheter and injections. Six days after her return, an examination was made per vaginam by M. Jourel and one of his friends, and the uterus was found in a state of retroversion. An attempt was made at reposition, without success ; this was repeated on the next day with a similar result—they now attempted to produce abortion by introducing a catheter into the mouth of the uterus, but the neck of this viscus was so bent, as to

\* Dictionnaire des Sciences Medicales, vol. ix. p. 81.

prevent the operation from succeeding. It was then agreed to adopt Dr. Hunter's method of puncturing the uterus, in preference to Gardien's operation of pubic synchondrotomy. M. Jourel operated with a common trocar passed along the fore finger of the left hand, so as to pierce the posterior part of the vagina. The canula transmitted about a pound of bloody fluid; the uterus immediately became softer, the pulse less frequent, and the general condition of the woman was improved. As the woman was much fatigued, immediate reduction was not attempted.

"From this time the urine was freely evacuated; the next day things continued in the same state—much serum oozed from the vagina—the uterus became a little painful, and narcotic injections and fomentations were prescribed for its relief. After this the pulse became smaller and more frequent; the belly was sore to the touch; the uterus harder and more tender. The difficulty of discharging urine returned in the forenoon; she vomited twice, and had discharges of flatus per anum. Her strength failed; the discharge was interrupted, but returned the next day; her stools thin. Towards evening the symptoms became milder, and continued to subside, but she remained still very weak. She voided her urine partly involuntary.

"Typhoid symptoms began to shew themselves, and there was a putrid discharge from the vagina—a copious and involuntary discharge of urine when in a vertical situation. Bark injections were employed. By the use of tonics her strength was improved; the fœtid discharge only took place at intervals. On the 27th of September, thirteen days after the operation, the uterus restored itself to its natural position. On the 2d of October it was reduced to its usual size. There was a discharge of a puriform substance from the anus, which gradually subsided, and ceased altogether on the 10th of the same month. The patient went into the country for three weeks; during this interval, and till the 15th of December, when menstruation was restored, she experienced a distressing tightness of the



abdomen, and some colicky pains ; but after the restoration of the catamenial discharge, she enjoyed good health."

The puncture of the bladder above the pubes, has also been performed with success in a case of retroversion, by Dr. Cheston.\* In this case the bladder was much distended, even above the navel ; the water was several times drawn off by the catheter, but at length a total suppression took place, and though a male catheter could be introduced two-thirds of its length, the bladder could not be emptied—the attempts at reduction failed ; the woman was about four months advanced in pregnancy—there appeared no other alternative but puncturing the bladder. This was accordingly done above the symphysis pubes, and the water evacuated ; the uterus restored itself, and the woman went her full time, was safely delivered, and has since been pregnant. Dr. Cheston suggests two cautions in performing this operation : one that the urine should be drawn off very gradually indeed, and the other that a short trocar should be used—he mentions a fatal case, where a long one was employed.

\* Med. Communications, vol. ii. p. 6.

STRICTURES UPON  
DR. MERRIMAN'S OPINIONS

OF RETROVERSION OF THE UTERUS AND EXTRA-UTERINE  
CONCEPTION.

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THE observations now presented form a sequel to those offered on "Retroversion of the Uterus," in No. 2, of the Philadelphia Journal of Medical and Physical Sciences, and originally constituted a part of them. I was induced to divide the paper from its great length to which it had unexpectedly run, and for which I have since been rejoiced, as I have been afforded the opportunity of reading Dr. King's "Analysis on the subject of Extra-uterine gestation and retroversion of the gravid uterus." Dr. King, in several instances, employs the same arguments against Dr. Merriman as I have used; I shall therefore, when this occurs, avail myself of his authority to support my own; and I shall do this with the more confidence, as Dr. K. agrees with Dr. M. in the belief of the possibility of the uterus remaining in a state of retroversion until the full period of utero-gestation, (though decidedly hostile to many of his "proofs;") while I cannot for a moment admit it. As my object in this paper is to elicit truth, I shall, in conducting it to issue, give every consideration to the arguments of Dr. M. they appear to merit; and where I differ from him, as I am obliged almost *toto cælo* to do, I trust I shall conduct my arguments with that liberality and temper that should ever characterise discussions of this kind.

Medical science has sustained great injury from overweening zeal, and the inordinate desire of generalising; cases, chancing to depart from their ordinary course, have with too much facility given rise to hypotheses to which the best interests of humanity have been sacrificed; and certain vague phenomena have laid the foundation for practices at variance both with experience and common sense. These positions, we think, are well illustrated by the principles which govern certain individuals in the treatment of the diseases we are considering; for instance, in the retroversion of the uterus, because, in a few instances, spontaneous reposition has taken place in the early months of pregnancy, it has been hastily concluded, that the interference of art was seldom or never necessary; and from two to three cases of an anomalous kind, but supposed to be retroversion at full time, and which, after long continued and terribly severe sufferings, did well, it is attempted to be established, that the uterus can remain retroverted until this period, and the woman be relieved by the efforts of nature alone; and as an inference from these very doubtful cases, it is declared, that the instances purporting to be cases of extra-uterine foetation are not such, but are truly evidences of retroversion at the full period of gestation, but where the uterus had suffered lesion either from rupture or ulceration, and thus permitted the foetus to escape into the cavity of the uterus, or to place itself between the vagina and the rectum.

In matters purely speculative, little injury, perhaps, is done by giving rein to the imagination, or forcing facts to square with a favourite hypothesis; but in practical matters too much care cannot be taken to guard against the vagaries of fancy, or the misapplication of facts. This in an especial manner should be avoided by those who give tone to public opinion, or can direct the public judgment. In this particular we fear that Dr. M. is somewhat reprehensible, as he appears to enjoy a reputation for talent and experience that would justify, perhaps, any one who might adopt his opinions. He seems to us to have admitted

with too little hesitation or scrutiny, circumstances that would, to say the least, admit of very different explanation, and deduced from them conclusions at variance with many well established observations. It is our design in this paper, to redeem the pledge that these assertions would seem to impose upon us ; in prosecuting which, we shall first consider the facts upon which Dr. M. has built his hypothesis, by commenting upon them in detail, either as related by himself, or referring to those authorities wherever there may be a material omission on the part of Dr. M. in the history he gives of them ; second, attempt explanations adverse to those of Dr. M. from the facts employed by him, or citing those which have occurred in our reading or observation.

Dr. M. commences, or rather introduces his subject by the following observations.\* “The records of medicine are abundantly supplied with reports of cases of extra-uterine gestation. These we may divide into two classes :—First, where the conception has been detained and nourished in some of the appendages of the uterus and the uterine system. Secondly, where the nourishment and maturation of the fœtus have been supposed to be effected in a receptacle separate and apart from the uterus and uterine system. This last opinion seems to have been adopted too readily ; at least it will, I am fully persuaded, be found, upon a nice and accurate examination of this latter class, that the writers of the cases were not so minutely exact in their observations, nor so explicit in the detail of many very important and essential particulars, as to stamp with credulity an occurrence so incomprehensible and so repugnant to the usual operations of nature.” The question may well be asked, whether every deviation from the natural order of things can be considered any thing more than an exception ? It does not necessarily imply an impossibility, nor can we believe all have been mistaken who have related cases purporting to be of extra-uterine concep-

\* Preface, p. v.



tions, or that they have "omitted important and essential particulars" to make their statements clear as to the points they wished to establish. What is there more extraordinary in a ventral conception than in fœtuses being found in the abdomen of both boys and virgins? \* Besides, the relators of these histories had no theories to support, and are consequently to be supposed free from all bias upon the subject. They may be supposed to have faithfully detailed what they saw, without a motive to disguise or mislead. If this be so, we cannot hesitate, without being chargeable with undue scepticism, to believe them to be unequivocally cases of extra-uterine conception. But if it could really be proved that there were occasional errors upon this subject, it is nevertheless certain that there have been indisputable instances of extra-uterine pregnancies; and this is sufficient to destroy the conclusion that these occurrences are impossible, because to Dr. M. they are "incomprehensible and repugnant to the usual operations of nature."

Dr. M. has espoused the notions of Dr. Denman upon the subject of retroversion; and it is important to the investigation we are about to make, that these opinions be briefly noticed, that the subsequent observations may be the better understood. In his theory of its formation, and his directions for its relief, he but repeats that celebrated accoucheur. We shall therefore not follow him in detail, as what we have said in our former paper, will apply as strictly to him, as to Dr. D. We cannot however pass in silence the following curious directions for the relief of this complaint. "Respecting the method of curing retroversions of the womb, enough has been said to show that the principal reliance is to be placed upon the introduction of the catheter; this should be done twice at least or oftener in the twenty-four hours. Care is likewise to be taken to keep the bowels open, and rest is to be enjoined. By pursuing this

\* Duncan's Annals of Medicine. Medico Chirur. Trans. vol. i. p. 136. Ibid. vol. vi. p. 124. Med. Repos. vol. xiii. p. 1.

plan steadily, the mal-position of the uterus is usually overcome in a few days. It is still customary with some practitioners of eminence, to make use of artificial means for replacing the womb, after the bladder has been emptied, and the bowels opened ; and there can be no great objection to make such attempt, provided it be done cautiously, and that no force be made use of. *In general, however, nothing of this kind is either necessary or advisable."*

Now, it is admitted by all who have written on this disease, that there is often a difficulty and sometimes an *impossibility* to introduce the catheter ; and Dr. Denman\* informs us, that "many women have lost their lives from the want of expertness in introducing it." "Expertness" can only be acquired by repeated opportunities ; yet a young practitioner, in many instances, must be the operator. If this be so, the woman agreeably to the directions of Dr. M. is exposed to greater risk from the mal-adroitness of her operator in the introduction of the catheter, than from the "force" which he so earnestly deprecates. Dr. Denman also acknowledges, "that there is an obstinate constipation of the bowels, produced by the pressure of the uterus upon the rectum, which renders the injection of a glyster very difficult, or *even impossible* ;"† and we know that in most of these cases, purgatives are totally unavailing—yet we are to draw off the water two or three times a day, and to keep the bowels open !

Dr. M. has no objection to reposition, "provided it be done cautiously," and without force. Now, force being a relative term, we cannot with certainty determine that exact degree of it, which would in the opinion of Dr. M. be objectionable. If the operation be attempted by a man well instructed in his profession,‡ no more "force" we

\* Introduction, vol. i. p. 141.

† Introduction, vol. i. p. 137.

‡ The most simple and the most necessary operation, in the hands of an ill-instructed and rude practitioner, may be followed by the most serious consequences—of the truth of this, the history of our profession affords but too many examples—and should an operation be proscribed, because perchance it may be ill conducted, we should have but few, in the wide range of human misery, that could be ventured on.

trust will be used than is necessary to the end ; and if this be well directed upon a uterus that is capable of reduction, it will not be so great as to do mischief ; and if less be employed, the end cannot be insured. Besides, it is well known that this operation has been very frequently performed, without any injury resulting from it. From this it is evident, that Dr. M. adopts, in the fullest extent, the opinions of Dr. Denman, upon the causes, mechanism, and mode of treatment of the retroversion of the uterus ; and we hope to make it equally clear, that it was this affliction, that led Dr. M. to the unsatisfactory explanations with which his hypotheses abound.

But let us recur to the main object of these "strictures." Dr. M. endeavours to prove by the history of several cases, "that the uterus which has become retroverted in the earlier months of pregnancy, may continue in that state till the full period of gestation has elapsed." And says, "whenever a retroversion of the uterus shall have taken place and have continued to the end of the utero-gestation, it may be expected that the case would terminate in one of three ways :

"First. The good form of the pelvis, the health and strength of the mother, and the efficacy and continuance of the pains, may all combine to replace the uterus and produce a favourable issue." In conducting our remarks, we shall consider the cases he offers as proofs of this opinion in the order Dr. M. presents them. He commences by admitting, with every writer almost upon this subject, that very serious consequences may result from the untoward situation of the uterus, called "Retroversion," but adds, "it is, however, consolatory to know, that, under some circumstances, the uterus may remain in a state of retroversion for a long time, even to the completion of the period of utero-gestation, without producing a total suppression of urine or any other uncommon or alarming symptom." He attempts to support this postulate by the relation of several remarkable cases, almost all of which are recorded *by their authors* as instances of extra-uterine

conceptions. He observes that the first case of this kind that was ever published in *proof* of such a fact, is recorded by Dr. Henry Seguin Jackson in a useful little work, entitled "Cautions to Women, &c." We referred to this work with the hope of finding in it a detailed statement of this case, and the reasons which induced Dr. Jackson to consider it an instance "of a woman reaching the full period of gestation with a retroverted uterus," but in this we were disappointed—for Dr. J. barely asserts "that he had an opportunity of seeing such a case, in company with Drs. Bland, Denman, Thynne, Merriman, and Croft;" and that "the situation of the woman at first appeared inexplicable, and she continued several days in labour, but the gradual efforts of *nature* at length completed her delivery, by restoring the womb *nearly* to its natural situation."

From this vague and unsatisfactory account, Dr. M. concludes this to be a case of retroversion continuing until the last period of utero-gestation. It may have been sufficient for him, who perhaps from personal knowledge of Dr. Jackson could rely upon his accuracy; but more is required by those who have not this advantage, especially as all *a priori* reasoning would consider this condition a physical impossibility. Dr. M. has, we believe, too hastily decided upon this case; and the more especially if he was in possession of no more of the circumstances than have been related by Dr. Jackson;\* it is true indeed that he again adverts to this case, which would lead one to the belief he was in possession of more than he makes meet the eye; but he neither details the case, nor indicates the authority for saying that this patient "never had an entire suppression of urine, but suffered severely from partial suppression and dysury, between the third and fourth months of her pregnancy." We would ask any candid man conversant in obstetrical practice, whether a "suppression of urine and dysury" characterise with sufficient

\* P. 26.



accuracy a retroversion of the uterus. Surely they do not; yet these symptoms are the only evidences offered the public of this being a case of retroversion of the uterus.\*

We here may again take occasion to lament, "that if a woman about the third month of her pregnancy, has a suppression of urine continuing for a certain time and producing a certain degree of distention of the bladder, we may be assured that the uterus is retroverted," should be considered as the diagnostics of this complaint; it has evidently betrayed Dr. M. into a very questionable conclusion, and shaken our confidence in both his candor, and the accuracy of his observations. We hesitate the more to admit this to be a case of retroversion protracted to the "full period of utero-gestation," as he immediately afterwards informs us that "as she advanced in her pregnancy, she was relieved from much of this inconvenience, *probably* by the parts adapting themselves to the situation she was in." Is it within the scope of common belief, that a retroverted uterus at the full period of gestation, can occasion less inconvenience than at the third or fourth month of pregnancy? Do not the histories of fatal cases declare exactly the contrary? The one related by Dr. Hunter† demonstrates, that even at the fourth month, the pelvic cavity can be so entirely occupied by the uterus, as not only to produce a suppression of urine, but to require great force, and a division of the symphysis of the ossa pubis, before it could be liberated from its confined position—can it then be for a moment believed, that a fully distended uterus, in a state of retroversion, would create fewer evils than one at the fourth month? We could easily multiply examples of this kind; but we are restricted by the fear, that this paper would run to too great a length. From what we have said, we think that Dr. M. is by no means entitled to the conclusion, that this was "a case of retroversion, at the full time of utero-gestation;" for we are

\* P. 27.

† Med. Obser. vol. iv. p. 400.

fully persuaded, that so much of Mrs. Wilkes's case as has been detailed by Dr. M. can be explained upon very different principles.

The next case related by Dr. M. in support of his opinion, fell under his own observation ; and if carelessly read, might be considered as a proof of his position ; but if strictly inquired into, will be found as equivocal as the one related by Dr. Jackson. Dr. M. informs us, that “ Mrs. F\*\*\*\* became pregnant for the first time, about September 1805. She did not suffer more during her pregnancy than most other women, except that for *the last two or three months she was troubled with difficulty of parting with her urine, and considerable pain in the act of passing it ; yet her sufferings in this particular were not so great as to induce her to consult her accoucheur upon the subject.* She neither at this time, nor at an earlier period of her pregnancy, experienced a total suppression of urine, nor does she recollect having ever retained it long enough to occasion any considerable inconvenience. When about five months advanced in her pregnancy, she was much terrified and afflicted on hearing of the sudden death of an aunt ; which, as she herself expressed it, seemed to turn her whole inside upside down ; and to this she imputes that alteration in the situation of the womb, which *she was given to understand either then, or at some other time, took place.*”

This case is remarkable for several of its particulars.—First. It is supposed that the retroversion took place at the fifth month of her pregnancy ; yet we are informed she never suffered any remarkable difficulty in passing her water, nor ever had a “total suppression ;” and indeed the symptoms did not take place until the “last two or three months of her pregnancy.” We would inquire what evidence there is, that the retroversion, if it really existed, took place at this period of gestation ? There is none—the woman's declaration, that the tidings of her aunt's sudden death “seemed to turn her whole inside upside down,” does not prove it produced retroversion of the womb. Besides, this is a familiar mode of expression with women

when suddenly alarmed, and really means with them nothing more than that flurry of spirits and hurry of circulation that is invariably consequent upon surprise; it is almost always accompanied with palpitation of the heart, and great commotion about the pericordia, and hence the expression. Second. This "turning upside down," was not followed by one symptom that we are informed of, that would lead to the suspicion of a retroverted uterus; the patient experienced no inconvenience from her urine until the two or three last months of her pregnancy, and then it was not so intense as "to induce her to consult her accoucheur." Third. The period at which this accident is supposed to have taken place, makes it liable to strong suspicion; for it seems admitted, that the bulk of the uterus at the fifth month\* is too great to permit this change to take place, unless there be some accompanying circumstance which may prevent the uterus from developing itself to the size which is usual at this period of utero-gestation—now, we are not apprised of any such circumstance, but are told on the contrary, that Mrs. F. "did not suffer more during her pregnancy, than most other women." Can we then for a moment imagine, that the uterus became retroverted at the time supposed by Dr. M., since had it happened at that period of gestation, the size of the uterus must have been such as to have filled the pelvic cavity, and at once given evidence that this change had really taken place? Or if it took place, she could not be so far advanced in her pregnancy as is supposed. Fourth. The patient herself attached at the moment no consequence to this alarm; for it was not until she was informed that a change had taken place in the situation of the uterus, that she adverted to it; "it then seemed to her the only mode of accounting for it."

Mrs. F. was taken in labour in June, which from the

\* "After the fourth month, its volume (the uterus) is generally so great that it cannot suffer such a displacement; because its height at that time, in most women, exceeds the breadth of the pelvis, taken from pubis to sacrum."

*Baudelocque, System of Midwifery, vol. i. p. 163.*

period it is stated she became pregnant, would make the full time of gestation.—She had considerable pain—no os uteri could be found, and the whole examination per vaginam, led to the belief of retroversion.—On the second day of her labour she was attacked with fever, delirium and convulsions—these all subsided by a bleeding. After a continuance of more or less pain for five days, there was “a mucous discharge tinged with blood” perceived, upon withdrawing “the finger from the ossa pubis,” which led to the conclusion “that the os uteri was situated in that direction.” “On the sixth day of labour a considerable alteration was discovered in the pains, and in the situation of the hard semi-globular substance which occupied the pelvis.” On examination, there was found between the ossa pubis and “body of the uterus,” a thick fleshy substance of a flattened form, which descended into the vagina, while the tumour began to recede; “in a few hours more the womb was restored to its proper situation, and the os uteri could now be distinctly felt coming down from a considerable height above the pubes, and the child’s head behind it.”

“As the flattened substance just mentioned came more within reach, it was perceptibly distended by some fluid during every pain, and as it approached the os externum, resembled, to the touch, the membranes distended with the liquor amnii, but those membranes much thicker than in the natural state. Shortly after some of the bones of the cranium became loosened, and were forced down into this bag or pouch; and now it became evident that this was only the scalp of a dead and putrid child, and that its distention was owing to the contents of the cranium being forced into it by the pressure of the pains.”

“The death of the child being thus clearly ascertained, Dr. Merriman (the uncle of the author) determined upon making an opening through the distended scalp, in order to evacuate its contents, which he received in a basin, *nearly to the amount of a pint. It consisted entirely of grumous blood and the brains of the putrid fœtus.*”



This case may be looked upon as a case of retroversion terminating in *abortion*; for we cannot agree with Dr. M. that it is an instance at full period, as he has not, in our opinion, substantiated it, either by the detail of the case or his explanations of it. Our reasons for dissent are, first, That no symptoms which would characterise this displacement, or even lead to a suspicion of it, existed until "the two or three last months of pregnancy," and they were not so severe as is usual with this complaint. Second. That we have no instance upon record where this disease was not accompanied with distressing symptoms, unless it be, perhaps, in the very early months. Third. That the symptoms which usually attend retroversion did not take place until (agreeably to the reckoning Dr. M. gives us) the sixth or seventh month of utero-gestation; a period at which Dr. M. himself would declare it would be impossible it should happen, unless some attending circumstance in the woman's health should prevent the uterus developing itself agreeably to the usual order of this process; but we are not informed of any such condition; but, on the contrary, it is expressly said, that "Mrs. F. did not suffer more than women usually do." Fourth. That were this a case of retroversion at full time, the pelvis should have been completely occupied by the enlarged uterus; but this was not the case, for there was a free issue to the fæces, and, for the most part, to the uterine.—Could this possibly be the case if the uterus were fully developed? At the third month in many instances, the suppression of urine, and the interruption to the discharge of the fæces, is complete. This happened in the case related by Dr. Bell.\* It obtained in many others we could relate; it was an attendant at the fourth month in Dr. Hunter's case,† besides others. Can it then be, for a moment, admitted, that neither of these circumstances should exist when the uterus had acquired three times the size as at these periods? Fifth. That it amounts almost to a demonstration that Mrs.

\* Medical Facts, vol. viii. p. 32.

† Med. Observ. vol. iv. p. 400.

F. could not be much beyond the fifth month of pregnancy, when we consider the size or capacity of the child's cranium, which is determined by its contents, "measuring nearly a pint," and consisting "entirely of grumous blood and brains."

Now let us suppose that one half of the fluid discharged by opening the scalp was brain, would the space occupied by less than an half pint be equal to that of a head at full time? Certainly not. There is another circumstance related in the history of this case, which, in our view, strongly corroborates the opinion we have just ventured to advance, (namely, that Mrs. F. was not in her ninth month of pregnancy at the time of her labour,) which is this: That "by applying the hand upon the abdomen, there could be very distinctly felt two tumours in the left hypogastric region." The uterus at full period, it is well known, would have reached considerably above the navel, and would remain there until the expulsion of the head from the vagina should oblige it to descend; and even at this moment it would scarcely be so low as the umbilicus. The two tumours which were "distinctly felt," were most probably diseased ovaria, and thrown over to the left side in consequence of the displacement of the uterus; and this conjecture is rendered more than probable from the following considerations:—First. That were it an uterus at full time, though retroverted, it could not occupy the "left hypochondriac region exclusively." This must be obvious to every one. Second. That these tumours were "felt to be lying so very near the surface," says Dr. M. "that we could hardly conceive *that the parietes of even a remarkably thin uterus* could be interposed between the integuments and the child." Third. Because it is immediately after stated, that "from feeling the fundus uteri through the rectum, we were led to believe, that the parietes of the uterus were of the usual thickness." Fourth. Because Mrs. F. Dr. M. tells us, "has not been pregnant since," making a period of five years\* since her delivery. This

\* Dr. Merriman's Essay, we are informed, was first published in 1810

last reason may not perhaps appear as conclusive as those urged above, since the uterus itself may have been injured, and thus prevent impregnation. This may be so ; but Dr. M. tells us " that no opportunity had offered to ascertain the exact state of the uterus ; but there is no reason to suppose that it sustained any injury from the awkward situation into which it was thrown."

The next authority Dr. M. produces in support of his opinion, is Deventer—in making use of him, we are sorry to say, he has made garbled extracts ; and has forced his meanings. Dr. M. has used just as much of his author as suited his views, whereas, had he given Deventer fairly, he would be decidedly against him. We shall therefore give extracts from this judicious author to prove what we have just advanced ; and we do this the more willingly, as his work is in the hands of very few in this country. In making use of Deventer's description " of a certain kind of Labour," all he says upon this subject, must be admitted to be, either correct, or incorrect ; if the first, we shall prove Dr. M. in the wrong ; if the second, he must not be considered as authority, and consequently Dr. M. will have no right to avail himself of his errors. Deventer's general accuracy is admitted by all ; and Dr. M. believes " his integrity has never been called in question"—what he relates then, must be received as the result of experience ; as he himself assures us it is ; he says, " I have found from experience that the womb may be too much resupinated, or incline with its fundus backwards, being forced against the backbone, so that its mouth or passage is not only raised too high into the abdomen, but is so obliquely situated, that its axis no longer corresponds with that of the vagina."

This account of the malposition of the uterus may be considered as a brief description of what Deventer considered a posterior obliquity of this viscus ; and which Dr. M. endeavours to show is a retroversion at full period. But we cannot find in the entire chapter upon this subject, any thing to confirm this opinion—Deventer is very particular in his

history of this supposed obliquity ; but there is no mention of a single symptom which would lead to the belief, that it is a retroversion he is describing under a wrong name.\* Dr. M. confesses "it is remarkable, that the authors who followed Deventer upon the subject of the obliquities of the uterus, *and who copy his description of this position, do not once speak of it as having occurred in their own practice or knowledge.*" It is indeed remarkable, that such silence should have been observed ; and we think this silence most decidedly proves that the retroversion at full period, (for such is Deventer's posterior obliquity, agreeably to Dr. M.) is of most rare occurrence, since neither Mauriceau, La Motte, Levret, Baudelocque, Smellie, Hunter, Denman, Sacombe, Spence, Hamilton, Gardien, Maygrier, &c. &c. give us any account of it.

We will however make a few extracts from Deventer to prove Dr. M. has garbled his account of the displacement he intended to describe—we shall quote Dr. M. so far as he goes, and supply such omissions as we think important, and place them in italics that they may be distinguished and compared with what Dr. M. has quoted to serve his purpose. Deventer says—"it sometimes happens that the midwife cannot touch the os uteri, or at least only a small edge of it, and that only when the os uteri is widely open," *for the child's head fixes it upon the os pubis, therefore the whole margin cannot be touched, since the finger can only come in contact with the lower part.* In this quotation Dr. M. has omitted a part of the text that is very important—for Deventer expressly says, that the child's head is pressed upon the os pubis ; now, this could not possibly be the case were it a retroversion, since in this case the fundus must be downwards, and the head free above the superior strait,

\* Chapman, it would seem, had met with cases similar to those described by Deventer ; for in giving the history of three women who had suffered from lacerated vaginæ, he observes, "I have been called to three women where, upon searching, I found the vaginæ quite broken through in the back part, by the midwife's rudely thrusting up her hand, in order to come at the opening of the womb, which in all these subjects happened to lie very forwards towards, and indeed in some measure, under the os pubis."—*Midwifery*. case xxx. p. 125.



and entirely out of the reach of the finger ; besides, one portion of the edge of the opened os uteri can be felt at the lower part—now in Dr. M's case above noticed which we believe was a retroversion, he declares that the os uteri could not be felt. “ In order to touch it then,” says Deventer, “ she must pass up her fingers cautiously, between the neck of the bladder and the mouth of the womb ; for if she introduces her fingers backwards towards the rectum, she will find nothing but a kind of bag or pouch, into which there is no opening ; if she presses a little forcibly against this, she may ignorantly conclude, she is feeling the head of the child, not considering that it is still enveloped by the uterus, and that she would in vain expect its descent. In this state of things the skilful midwife may discover, near the neck of the bladder, something like a semi-circular margin ; this is the edge of the os uteri—here, *if her fingers can reach high enough*, the midwife may even feel the hard globular part of the child's head, and the soft part, or aperture of the vertex ; whence it may be collected, that the body of the child, together with the uterus, is pressed too much against the back bone.”

We were not a little surprised that Dr. M. should have employed the latter part of this description, since it alone, without any of the context, is sufficient to destroy his hypothesis. Let this be compared with Dr. M's account of Mrs. F's case, and it will be seen that they do not agree in any one particular. In Mrs. F's case, “ the os uteri could not be felt,” owing to the vagina being wholly occupied by “ a large hard semi-globular substance.” In Deventer's account the os uteri can be felt, and no mention is made of any substance filling up the vagina. In Mrs. F's case the finger might be passed between the hard substance which was in the vagina and the ossa pubis, as high as “ it could reach, without discovering any traces of the os uteri.” In Deventer's the head of the child was “ fixed upon the os pubis,” and a portion of the circle of the mouth of the uterus could be touched. In Mrs. F's case the nature of the presentation could not be ascertained, as the situa-

tion of the os uteri was unknown to Dr. M. ; for he was obliged to "some mucus tinged with blood" for the belief that it was "situated behind the ossa pubis," as "it was impossible, by the most particular and careful examination, to discover the os uteri by the touch." Besides, in Mrs. F's case "the semi-globular substance was, by the force of pain, made to advance near to the perinæum," and could, by introducing a finger into the rectum, be "more distinctly traced." In Deventer's account we are distinctly told, that if the finger be carried towards the rectum, it would find nothing but a little close bag ; and if pretty strongly pressed, the child's head could be detected through the parietes of the womb, and which the midwife would in vain wait for its descent. In Mrs. F's case the idea conveyed by touching per anum "was, that the fundus uteri, containing either the head or nates of a child, had fallen down between the vagina and rectum ; thus filling up almost the whole space of the pelvis." In Deventer nothing analogous is to be found ; but, on the contrary, he tells us, and which Dr. M. takes care to suppress, "that from the situation of the uterus above described, the head, though never so well presenting, falls upon the ossa pubis," against which, by the force of pains, it is violently pressed, where its soft head sticking, is in some measure crushed, so that, *not being able to fall into the cavity of the pelvis*, the mother can by no means bring forth the infant, unless the head be removed and brought into the cavity of the pelvis.

In the whole of Dr. M's history of Mrs. F's case, it is evident that neither the os uteri, nor the presenting part, could be detected : yet Deventer tells us that the head, or even the fontanelle of the vertex can be felt. Dr. M. has made this part of Deventer very obscure by his translation, by affixing a condition not intended by his author. He makes Deventer say, "Here (the edge of the mouth of the uterus) *if her fingers can reach high enough*, the midwife may even feel the hard globular part of the child's head, and the soft part, or aperture of the vertex ;" whereas De-

venter merely states "that if she (the midwife) penetrates to the edge of the uterus, then the head, &c. may be felt!" It was important to Dr. M's hypothesis that the possibility of carrying the finger sufficiently high to touch the head should be called in question, since it was impossible for him, in Mrs. F's case, to do so: yet it is evident, that, could he have reached *far enough* behind the ossa pubis, he must have encountered the os uteri. It was therefore of consequence to reduce Deventer's case to the same condition with his own.

It is moreover certain, that Deventer meant the body and legs of the child were up in the abdomen; for he says, that from this (the account just given of the situation of the child's head,) we may be certain that the infant, as well as the womb, are too strongly pressed against the dorsal spine. Now this could not be, if these parts were not above the superior strait of the pelvis—but to put this matter beyond all dispute, let us refer to his 34th, 35th, and 36th plates; in all of which, the trunk and inferior extremities are represented as at the fundus of the uterus, and that made to rest against the lumbar vertebræ. It is only surprising that Dr. M. with these plates before his eyes should have attempted to force Deventer's opinions to quadrate with his own. We might add to this, that Deventer's directions for the remedying of this malposition, confirm what has just been advanced—they are too long for insertion, and we beg therefore merely to refer to them.

Dr. M. says, "these extracts" (namely those quoted from him,) "tend to show, that Deventer was describing a wrong position of the womb, under the denomination of an obliquity of the uterus, which was in reality a retroversion of that organ; for, how could the passage of the finger towards the os sacrum, be prevented by a substance in the vagina, feeling like the head of the child, while the os uteri was almost out of reach above the ossa pubis, but by a retroversion of the uterus?" To this we answer, that Deventer makes no mention of any substance in the vagina which would obstruct the passage of the finger towards the os sa-

crum; on the contrary, he says, "that if the midwife should pass her fingers in that direction, she would meet with *nothing but a little close bag*;" now this "close bag" is the vagina drawn forward from its natural position by the unto-ward situation of the womb, "which is raised *too high in the belly*, and is so obliquely situated, that its mouth will no longer be in a right line with the vagina." But if this "close bag" be pressed against, the head of the child can be felt through the parietes of the uterus; in Mrs. F's case, however, the presentation could not be discovered by the most deliberate and careful search; there is nothing then in common between these two cases.

"Or how," continues Dr. M. "could the os uteri be so situated, as to make it almost impossible to touch it, unless the fundus was thrown down between the rectum and vagina?" To this it may be replied, that Deventer expressly declares, that the uterus was too high in the belly, and too strongly pressed against the back bone, in the case he is describing, and that this is the reason the mouth of the uterus is carried up to the ossa pubis—and to prove, that he considered this to be the situation of both fetus and consequently uterus, we must again refer to the plates. If Deventer is describing from imagination, Dr. M. is certainly not authorised to employ to his advantage the fruits of it; his testimony must be received, as we have already said, either entirely or not at all.

"Or what," adds Dr. M. "except this position of the womb, could occasion the very great danger, which Deventer so feelingly laments?" If the position assigned to the uterus be granted, we can very easily imagine from whence the "danger" would arise—the head resting upon the margin of the pelvis at the ossa pubis in the manner described, it can be readily conceived, that both mother and child would be in jeopardy, since the uterine forces would tend but to confirm its situation there; the head would not descend into the cavity of the pelvis (and Deventer declares it will not in the case he is describing,) and consequently the woman cannot be delivered without experienced aid.



"If the fundus of the gravid uterus," continues Dr. M. "at nine months merely rested against the back bone without being retroverted, how is it possible that the os uteri should rise out of reach towards the cervix vesicæ?" To this we must first observe, that if the uterus were really retroverted, its fundus could not possibly rest against the "back bone," for it would necessarily be too low, and in an opposite direction—for it must in that situation, agreeably to Dr. M's own account, occupy the lower part of the pelvis; for he states that it can be felt through the rectum—now, the fundus cannot occupy both the hollow of the sacrum, and rest against the "back bone" at the same time: and secondly, we must declare, that Deventer in no instance says the os uteri is "beyond reach," but on the contrary tells us its lower edge can be felt, by carrying the finger between it and the neck of the bladder.

Dr. M. says, "It might indeed" (the mouth of the uterus) "be turned more than usually forwards towards the pubes; but could not possibly *ascend out of reach* from this cause." Now to this we need but repeat what we have this moment said, that Deventer does not say it does "ascend out of reach." "Besides," says Dr. M. "if this alone were the case, what should prevent the labour pains from restoring the os uteri to its proper situation in a few hours? This position of the womb might, indeed, occasion a tedious and hard labour, but could scarcely expose the life of the woman to much danger; whereas Deventer represents the situation of the womb as most *perilous*, in which both mother and child, after suffering a very long and painful labour, often lose their lives." To the latter we say, that when the inflection of the lumbar column is greater than ordinary, of consequence the projection of the sacrum increased, even without injuring the diameter of the upper strait from pubes to sacrum, the head of the child, if this should be the presenting part, will be made to rest upon the anterior margin of the pelvis, owing to the uterus rising higher, or rather, perhaps, not being able to sink into the pelvis as it is wont to do in a healthy construction of

these parts ; for the axis of the fundus and the mouth of the uterus, in this case, will not correspond with that of the superior strait, and this is precisely what Deventer urges to be the situation in those labours he “ so feelingly laments.” Should, then, this want of correspondence of the axes of the uterus and pelvis obtain, as we have just stated, it must be evident, that both mother and child will suffer greatly, unless timeously relieved. We must, then, from what we have said, conclude, that Deventer’s account of the posterior obliquity of the uterus affords no aid to Dr. M’s hypothesis.\*

We shall now proceed to consider Dr. M’s two other modes of termination of retroverted uterus at full time.

“ Secondly. The want of some or all of these fortunate circumstances, or injudicious management during labour, may occasion the poor woman to fall a victim to this untoward position of the womb in the course of a few days, either by a rupture of the uterus producing speedy death, or by an active inflammation or mortification of the parts.”

“ Thirdly. The uterus being unable to extricate itself out of the awkward position into which it was thrown, may passively submit to the burthen, until, by the slow process of ulceration, the fœtus may be excluded through the rectum or vagina, and the mother remain alive.”

Dr. M. considers these two last terminations pretty much together, thinking the facts and arguments which may support one will aid the other. We shall follow him in the march he has laid down for himself. He begins with the history of a case from Patuna, and we must take it for granted he relates the case fairly, as we cannot lay our hands upon the original. He says, “ The first which occurs to me as being of this kind is related by Nicholas Patuna,†

\* Dr. King concurs with Dr. M. that the description given by Deventer “ of some obliquities of the womb, were in whole or in part, cases of retroverted uteri.” Analysis, p. 47. We must really suppose that Dr. K. has rested satisfied with Dr. M’s extracts, and not have read for himself, by his consenting to such a conclusion.

† There is a case circumstantially related in the fifth volume, page 162, of the *Medico-Chirurgical Journal*, which is said by Professor Santorini to have happen-

a surgeon of Venice, of a lady from whom he extracted, through the anus, the body of a child which had been retained twenty months in the abdomen of the mother. The circumstances which induce a belief that this was an instance of retroverted uterus are, *the suppression of urine in the early part of her pregnancy, and the great discharge from the uterus of serous fluid tinged with blood during the ineffectual pains of labour.*

"This lady had borne two children at the full time, and had one miscarriage. In October, 1724, she conceived again, and in six weeks after, she was attacked with violent pain in the lower belly, and was unable to pass her urine except when lying on her back. From this time until July, 1725, when she entered her ninth month of pregnancy, she suffered much at various times, from pains in the belly and loins, and other complaints. At the beginning of this month she had a profuse flow of milk, which ran out and kept her very uncomfortable. About the middle of the month, her reckoning being then completed, labour pains came on, and the birth of the child was every mo-

ed in his own practice, but which we strongly suspect is the same as is related by Patuna. They agree in every particular, even to the accident which befel Patuna's patient on her journey to Venice, namely her receiving a severe shock from "the fall of her vehicle." It would seem a matter of downright impossibility, that two cases should so strongly resemble each other in their detail, not only as regards the main objects of the relation, but in all and every peculiarity attending the case itself. Patuna's patient was pregnant for the fourth time, so was Santorini's. Patuna's patient suffered from abdominal pains and dysury, so did Santorini's. Patuna's patient suffered much until her ninth month, so did Santorini's.—At this time in both, pains resembling labour came on, and the fœtus died most probably, and there was a plentiful secretion of milk, &c. In both there was a discharge of blood from the uterus, which became white and offensive. After a variety of sufferings, in the account of which they both agree, her health began to mend, when, they also both agree, she became again pregnant. About the sixth month, they both say, the lady suffered from the breaking down of her carriage, and pain and much distress followed; that upon examination per anum there was discovered a hard sharp substance, which proved to be the parietal bones. They were extracted, and, after a time, the remains of a *female fœtus*. In about eight days after this, they both say, the placenta came away, which was both putrid and small; and both agree, that, in a short time after, the health of the lady was restored.—See Merriman, p. 44, and Recueil Period. de la Soc. de Méd. de Paris, Aout 1817.

ment expected, but was not accomplished. Four or five days after this there was a great discharge from the uterus of a serous fluid tinged with blood, very putrid and offensive; and a draining continued many months after."

"Some months after, her general health began to improve, and she again became pregnant in July, 1726, and so continued until January, 1727, when she miscarried of a male child of the usual size at six months." "Being recovered from this miscarriage, she undertook a journey to Venice, during which she received a severe shock of the whole body from the carriage breaking down."

"After this she suffered considerably from fever, pain diarrhœa, vomiting, &c. At this time she was first visited by Patuna. He examined her per vaginam; and found at the superior part of this canal a tumour which was hard and not very large. He examined also per anum, and there found a hard pointed substance which scratched his finger, which was cautiously extracted by a pair of small forceps. This proved to be one of the cranial bones of a fœtus. In a few days more he succeeded in delivering in one mass all the remaining portion of a female fœtus. Eight days after the placenta was excluded, very putrid and much diminished in size. The mother soon recovered."

Dr. M. confesses that "the suppression of urine in the early part of the pregnancy, and the coloured discharge at the full period, when an effort was made to relieve the patient," were the circumstances which induced him to believe this to be a case of retroversion;—there is an old saying, "we can easily believe what we wish to be true," and on this principle we reconcile Dr. M's facility of credence. We have, we trust, already said enough in our former paper on the uncertainty of these marks to characterise retroversion, to convince any unprejudiced mind, that they are not to be exclusively relied upon. And we could cite many authorities to prove, that suppression of urine is a common symptom in pregnant women, and that retroversion is comparatively a rare one; and that this inconvenience is frequently produced in the unimpregnated



uterus by a prolapsus; and this and other causes may induce these symptoms in the early part of gestation, besides a retroversion; but as these facts are familiar to every obstetrical practitioner, we will consume no more time, in farther insisting upon them. As regards "the great discharge from the uterus of serous fluid tinged with blood during the ineffectual pains of labour," we can readily show this to be no uncommon attendant upon indisputable cases of extra-uterine pregnancy, and by no means decidedly marking a fœtus in utero.\*

The changes which the uterus itself undergoes in cases of extra-uterine conception, are similar in many instances to those produced when the fœtus is lodged within its cavity—that is, there is an increase of size, by a distention of its parietes, from an augmented influx of blood and a production of decidua, &c. Dr. Baillie† says that Dr. Hunter had a preparation of a tubal pregnancy in which the uterus was enlarged to twice its natural size and contained a decidua. Mr. Burns‡ says, "it is curious to observe, that generally the uterus enlarges somewhat in cases of extra-uterine conceptions, and in most instances I imagine decidua is formed." Dr. Clarke§ says, he found in a case of extra-uterine conception of two months, the uterus as large as if it contained the fœtus, and that decidua was formed. In the history of the case related by Mr. Blizzard,|| it is stated that "changes had been produced in the uterus, similar to what are seen in ordinary gestation; its parietes were thickened; its cavity enlarged; its cervix shut up with a jelly; and the glutinous effusion just described might not perhaps improperly be considered as a membrana decidua in its early state of formation." It would be easy to multiply testimony on this head were it important to do so—but, as we consider the production of the decidua indisputable, we shall rest satisfied with those

\* Dr. King also accounts for the dysury, &c. in this case by supposing the uterus was prolapsed, p. 49, Analysis, &c.

† Phil. Trans. vol. vii.

‡ Principles of Midwifery, p. 192, 5th edition.

§ Trans. of a Society, &c. vol. i. p. 216.

|| Edin. Phil. Trans. v. p. 189.

we have now offered. It would seem then easy to account for a coloured fluid issuing from the uterus when the organisation of this membrane should be injured, as it would necessarily be, by repeated uterine contractions, and also for its continuance for a long time, even after pain had ceased, without supposing it essential to these ends, that the fœtus should be contained within its cavity. The vascularity of the decidua is put beyond doubt by injections; and the phenomena of abortion furnish further proof.

Besides, we have evidence of this discharge taking place in unequivocal instances of extra-conception. In the case related by Mr. Turnbull,\* at the end of seven months, there was a flooding which continued for four or five weeks, and a substance was discharged which was taken for placenta by the midwife, but which most probably was nothing more than the expulsion of the deciduous coat. In Mr. Hey's† case there was flooding at the ninth month; and continued for a month afterwards to be serous. In a case related by Dr. Lobstein,‡ at the end of the third month, coagula were discharged, then fluid blood, and then coloured serum. La Croix§ relates a case in which the discharge continued for a long time from the uterus. Lest it should be supposed, that certain of the symptoms which accompanied this case, were peculiar to uterine pregnancy, we shall advert to them, and prove by incontestable evidence, that they are necessary, or at least usual attendants upon extra conception. And, first, of the "profuse flow of milk from her breasts, which ran out and kept her very uncomfortable." It would seem from the history of very many cases of indubitable extra-uterine conception, that this is almost a never-failing attendant, and is recorded as such, wherever the instances are well detailed; and marks in these cases, as well as in those of uterine pregnancies, the death of the fœtus.|| It is almost an infallible guide in

\* Mem. Med. Soc. vol. iii. p. 176.

† Med. Obs. and Inq. vol. iii. p. 341.

‡ Med. and Chir. Jour. vol. iii. p. 415.

§ Fourcroy, *Medicine Eclairée*, tom. iv. p. 346.

|| Ibid.

the latter kind, and we are disposed to believe it to be equally decisive on the other.

In Mr. Turnbull's\* case, the breasts became hard and painful, and a butyraceous fluid was plentifully secreted. Mr. Hey† reports his case to be accompanied with a secretion of milk. In Mr. Bell's‡ case, milk was secreted, and continued to be secreted for several years. It would be easy to add to this evidence were it important. We may next notice several symptoms common to pregnancy, such as pains in the belly and loins, vomiting, &c. but all of which attend at extra-uterine conception. Indeed, the cases resemble each other so much, that it requires an examination per vaginam to determine the question. The quickening is also as evident in many instances of extra-conception as in the uterine; but in the former, it is generally described as higher or confined to one side, &c.

Let us now advert to one very important part of this history, namely, "that she again became pregnant in July, 1726, and so continued until January, 1727, when she miscarried of a male fœtus, of the size that is usual at six months." This circumstance, if the case be regarded as one of the extra-uterine kind, is not very uncommon, for we have a number of similar instances upon record. Dr. King§ relates a case in which this occurred, at the end of six years after the first conception, and during the sojourn of that fœtus in the abdomen. In Dr. Bard's|| case, there was an uterine conception and delivery of a healthy child while a fœtus occupied the abdomen; besides many others. In several of these, the uterus is described after dissection, to be healthy and without blemish in its appearance. Now, were this a case of retroversion, as supposed by Dr. Merriman, and were we to adopt his explanation of such cases, we must believe the uterus capable of sustaining, without the interruption of its most important functions, injuries of the most extensive and destructive kind. It would be

\* Mem. Med. Soc. vol. iii. p. 176.

† Med. Obs. and Inq. vol. iii. p. 341.

‡ Med. Comment. vol. ii. p. 72.

§ Edin. Med. Essays, vol. v. p. 362.

|| Med. Obs. and Inq. vol. ii. p. 369.

admitting it could bear the loss of large portions of its substance without any derangement of its economy. The process of ulceration, it would seem, could scarcely injure it, since it can take place to such an extent as to allow a fœtus of a size that the sex could be discovered, together with its placenta, to pass through it, without destroying its future usefulness. Can we, or can Dr. M. himself, for an instant believe, that the uterus can sustain the loss of so considerable a portion of its fundus or body, and yet perform its functions as certainly and as healthily as if its integrity had never been impaired? We cannot then regard this case of Patuna's to make any thing in favour of Dr. M's explanation.

Dr. M. quotes a case from Bartholine, but on which he himself seems to lay but little stress; yet, in our opinion, rather more than it deserves; for although Bartholine may have thought as he has written, still it is an opinion founded upon most slender and equivocal data. Bartholine is made to say, "there could be no doubt that it" (the fœtus) "was detained in the uterus, as there was a discharge of pus from the womb at the same time, and pains like labour had been felt about the pubes and groins."\* In an investigation which involves so much, we cannot forbear expressing our surprise, that Dr. M. should have employed evidence so imperfect and doubtful; and the more especially if the facts were really what Dr. M. would wish them to be, still, they would prove nothing to the purpose. How was it ascertained that the discharge of "pus" was really from the uterus? It is scarcely to be believed that Bartholine took the trouble, by the employment of a speculum, to determine that the discharge really issued from the uterus; and if he did not, the source of the discharge may have been from the vaginal surface;† or the abscess formed

\* Bartholine does not by any means speak so positively on this subject as he is made to do by Dr. M. He merely states, "it would seem," &c. instead of saying "there could be no doubt," &c.

† The discharge from leucorrhœa so much resembles pus in many instances, that it would be difficult to discriminate between them; or the surface of the va-



by the irritation of the bones between the rectum and vagina, may have opened itself within that canal, since there would be no more difficulty in penetrating this way, than through the rectum. And as regards "pains like labour," it is well known that they attend cases of indisputable extra-uterine conceptions.

Dr. M. next brings a case from Perfect's cases in Midwifery, vol. ii. p. 171, where Dr. M'Kensie says, "I knew an instance of a child found *without the uterus in the abdomen of the mother*. The pains came on, and a midwife was employed; this woman, finding an enlargement in the vagina, mistook that for the membranes, which she attempted to break through by repeatedly scratching it with her nails, in which she succeeded so far as to evacuate the waters. However, the birth of the child being still retarded, a man midwife was procured, but to no purpose, the woman growing worse and worse, till at length she died."

This case is still less to the point than the one just mentioned from Bartholine—for Dr. M'Kensie declares "*the child was without the uterus in the abdomen of its mother*," consequently could not be a retroversion. But Dr. M. supposes Dr. M'K. was mistaken, we presume, from his having taken notice of this case, and most probably from the circumstance, that the midwife by scratching had "succeeded so far as to evacuate the waters." But is it reasonable to suppose that the midwife could simply, by the use of her nail, have scraped through the vagina and uterus? We should regard her penetrating through the vagina and membranes which cover the child, a work of considerable difficulty, yet practicable, and in doing this, give issue to some fluid which may have immediately surrounded or touched the child; for in cases of acknowledged extra-uterine conceptions, the fœtus is surrounded by more or less fluid. In Dr. Giffard's\* case a discharge of bloody water took place from the anus, before the child was delivered, which he ex-

gina may have secreted perhaps a genuine pus, or the uterus itself be subject to a purulent excretion, as suggested by Dr. King. Analysis, p. 58.

\* Midwifery, case 167, p. 375.

pressly declares to be "the water in which the fœtus floated."\* The mere discharge then of water is no way sufficient to throw any doubt upon the nature of Dr. M'K's case.

The next case Dr. M. presents us with, is from Dr. Simpson.† On this case, his remarks we conceive are not justified by the detail of it. He begins by saying, "I think it not *improbable* that the case which occurred to Dr. Simpson was a case of this kind," (retroversion at full time.) Dr. S. informs us, "that the woman had once been delivered by the crotchet." This woman, Dr. S. states, had been in labour for several days, and was with considerable difficulty delivered, after opening the head, owing to the deformity of the pelvis. "In her second labour," continues Dr. M. "he could not find the smallest opening into the uterus, and, *therefore*, judged that the lips of the os uteri had grown together." "But what could have occasioned this coalescence," says Dr. M. in a note, "of the os uteri, is not explained; there is no account of any inflammation about these parts after the conception (the second he means) had taken place."

Dr. S. is unfairly quoted in this passage by Dr. M. He makes Dr. S. draw the conclusion "that because he could not find the smallest opening into the uterus," it must therefore follow "that the lips of the os uteri had grown together." We shall show, however, from Dr. S's own words that he drew no such inference; but that the declaration that the lips of the os uteri being grown together, it was from ocular demonstration. This woman became pregnant a second time, and at the ordinary period had the symptoms of labour; which, says Dr. S. was hard "for two days before I saw her." He was then informed by the midwife that the inner orifice had yielded nothing; I left her half a day, and things remaining in the same way at

\* Dr. King, analysis, p. 10 and 11, tells us in his case, that "the instant the vagina was laid open, the waters flowed abundantly, the membranes being laid open with the same incision."

† Edin. Med. Ess. vol. iii. p. 241.

my return, I examined her condition, and found that the os tinæ had not only not yielded, but that the sides of it were grown together without any vestige of a passage; whereupon I asked the assistance of another physician, and Dr. Haddow being called was satisfied, as well as the midwife, that the case was such as I judged it to be." It was now agreed that an opening should be made into the os uteri by incision; but before this was done the vagina was dilated by means of a speculum; which enabled them "distinctly to see the cicatrice of the grown together parts." From this we find that it was not conjecture but demonstration.

It is true, as Dr. M. states, that "there is no account of any inflammation about these parts after the (second) conception had taken place," but there is a very particular one, of both inflammation and suppuration following the first delivery, and we have no hesitation to believe that this produced the union of the lips of the os tinæ. Dr. S. informs us that this woman "had for several days after delivery passed a great many small rugged stones by the urethra, and at length after her urine had been stopped some time, her husband drew out of the urethra a large piece of thick membranous substance, three inches in length, and in some parts two inches broad; one side of it was covered with a crust of small sharp-stones, the other side was inflamed and bloody." "The patient continued a long time with a plentiful suppuration about the pudenda." In this account we hear of sufficient inflammation, &c. to account for the agglutination of the mouth of the uterus. But it may be said by others, beside Dr. M. that this could not have happened at the time we suppose, since the woman became again pregnant, which could not have happened had the mouth of the uterus been permanently closed. But this is all speculation, and founded upon an opinion, that has in our opinion been satisfactorily proved to be erroneous,\*

\* It is very generally believed, that the male semen must be directly conveyed into the uterus that impregnation may take place; but this is abundantly disproved by Couper, Saumarez, Haighton, &c.

and this led Dr. M. into the persuasion that the inflammation must be posterior to conception to have produced this coalescence.

“As it was impossible,” continues Dr. M. to “find any opening into the uterus, it was determined, in consultation, to make an incision with a scalpel through the part where it was supposed that the os uteri was situated.” From this it would unquestionably be supposed, that Dr. S. had determined to make an incision at random; but when he speaks for himself, he appears certain of the nature of the part on which he is about to operate; for after stating, as mentioned above, that the cicatrice which united the edges of the os uteri could distinctly be seen, he adds that “he could have easy access to it, and divide it; which,” he says, “I did by an incision at least half an inch deep, before I pierced through the substance of the womb; then immediately introducing my finger at this wound, I touched the child’s head, and felt the whole circumference of the passage hard like cartilage, which yielded nothing to several throes she had after the incision, so that I was obliged to guide a narrow-bladed scalpel with my finger, to make several incisions into this cartilaginous ring.” Here we have every evidence that it was really the os uteri that was operated on, and not upon a part where “it was supposed that the os uteri was situated.” For it is first declared, that the cicatrice was seen which united the edges of the os uteri; secondly, he speaks of the circumference of the passage; thirdly, he calls it a cartilaginous ring. This indurated condition of the os uteri is recognised by several writers upon midwifery. Baudelocque speaks of it in the following manner: “Sometimes the pad which constitutes the neck of the uterus in the latter periods of pregnancy and in the time of labour, is hard, scirrhus, incapable of any extension or dilatation, so as entirely to hinder the exit of the child.”

Dr. King concurs with Dr. M. that this was a case of retroversion; but his reasoning on this subject we conceive is liable to great objection. He says, “The cartilaginous



hardness mentioned indicates that something more than the coats of the vagina were cut through. The convoluted vessels of the uterus might have given this sort of resistance to the knife; the peritonæum could not, unless slightly ossified." It is truly so, that "more than the coats of the vagina were cut through," or rather they were not cut through, but the uterus itself; and agreeably to Dr. K's confession, the convoluted vessels of this viscus might give this sort of resistance; though we are entirely at a loss to comprehend how "convoluted vessels" should offer more resistance than a firm elastic membrane. But as we are persuaded that the uterus itself was cut, we shall content ourselves with his concession, that its convoluted vessels might "give this sort of resistance," without any inquiry into his meaning; for to us, we are free to confess, it is totally unintelligible. He adds, "I concur with Dr. M's induction, viz. that this was a case of retroversion, because the patient died in twenty-four hours, a result which we trust would not accompany the incision of the simple vagina."

Dr. M. as well as Dr. K. has attempted to take advantage of the speedy death of the patient; for he sneeringly observes, "It can hardly be necessary to add, that the mother died in twenty-four hours," (after the operation.) Dr. M. had previously condemned it, as "being performed in a most rude and slovenly manner," of which there is not the slightest evidence from Dr. Simpson's account of it. We have read this case with great care, and we cannot for a moment admit that the operation (that is, the division of the os uteri) had the smallest agency in her death; and we are the more especially confirmed in this, by Dr. Simpson's statement of the sequel. He tells us, "My patient, immediately after being put to bed, was seized with pleuritic pain, very high fever, and difficulty of breathing, which, coming on so soon after her being fatigued several days with hard labour, during which she slept none, *but drank much of every thing in the way*, appeared to me rather the cause of her death, than any consequence of the incisions I

had made." In this we heartily concur with Dr. S.; for it is a fact well known to accoucheurs, that the mouth of the uterus has been frequently divided without the slightest injury having resulted from it. Coutouly proposes it as a safe practice in cases of convulsions, &c. where the os uteri refuses to dilate, and where it is highly important that delivery should speedily take place. He gives a number of cases to prove the efficacy and safety of his practice. See his *Memoire sur l'incision des Bords du Col de la Matrice, &c.*

"Now," continues Dr. M., "in this history one circumstance is mentioned, which goes strongly to prove that there was a retroversion of the womb. Dr. S. states that the liquor amnii made its escape early in the labour; and he imagines that this evacuation was effected through the urethra, between which and the uterus he supposes there was an opening, the effect of a former inflammation of the parts. Is it not much more probable that the liquor amnii was evacuated through the os uteri, tilted very high above the pubes, as in Mrs. F's case; and that the incision which Dr. S. made was not through the coalesced os uteri, but through the posterior part of the vagina and the fundus uteri?"

We will let Dr. S. explain for himself; and this will show how determined Dr. M. was to force this history to comport with his hypothesis. "The want of waters (at the time of cutting into the uterus) was some surprise, till I recollected that in the time of labour she told us they were passing, at which time I had the curiosity to make a strict observation, and found that, what she called, the waters, passed by the urethra, which opened by three orifices; this with her having lost such a portion of the bladder formerly, and her being subject to the gravel, gave me ground to think there was some communication between these passages and the cavity of the uterus, above the os tincæ." Dr. S. it is seen, speaks positively as to the issue of water through the urethra. We are led to believe it was ocular evidence, and therefore he could scarcely be mistaken; and

besides, his explanation is every way satisfactory of the probability of a communication between the uterus and bladder; since we could cite instances to prove, in cases of extra-uterine conception, that a communication has been made between the sac containing the fœtus and the bladder, and the bones of fœtuses have been passed through the urethra.

As to Dr. M's conjecture that "it may have passed through the os tincæ," which he *imagines* was tilted very high above the pubes, there is not the smallest foundation. In the whole history of this case there is no mention of the os tincæ not occupying its usual situation,\* nor of any tumour filling up the vagina, as must necessarily have happened had it been a case of retroversion; nor is there the most remote resemblance between this history and that of Mrs. F's.

The next case in order is Mr. Kelson's† which Dr. M. relates, with a suppression of two or three material points—we shall therefore give it from Mr. Kelson's own account.—He was sent to visit a Mrs. Townsend, who was suffering very considerable pain from a partial suppression of urine and fæces, and who supposed herself ten weeks with child, as she had missed two menstrual periods, "a very trifling appearance at the end of each month excepted." These symptoms were twice relieved by cathartics—in the course of a few days more, a total suppression of urine took place, and "upon examination," says Mr. K. "I discovered, as I then supposed, that it arose from a retroversion of the uterus, the lower part of the pelvis being completely filled with a hard tumour, and the os tincæ not to be found without much difficulty. I relieved her by the common female catheter, to which I was obliged to have recourse for a fortnight longer. At the end of that time, the impediment, as I had foretold, was suddenly removed, the uterus taking pretty much its natural situation."

\* On the contrary, we are informed by Dr. S. that he really saw the os tincæ; now this would have been impossible were Dr. M's conjecture well founded.

† Med. and Phys. Journal, vol. xi. p. 293

From this relation, we are of opinion, that the two first inconveniences which Mrs. T. suffered were not occasioned by retroversion; and were nothing more than what is pretty frequent with women in the first periods of pregnancy, from prolapsus, and especially with those who are much upon their feet, and subjected to more or less fatigue, as was most probably the case in this instance, as the patient was the wife of a farmer; and particularly as she was in both instances relieved by cathartic medicines. And, that total suppression did not take place until the vaginal tumour had acquired sufficient bulk to entirely compress the neck of the bladder: but which compression was eventually suddenly relieved by the tumour still augmenting, and rising higher in the pelvis, and thus freeing the bladder from the restraint which it had before imposed upon it, by pressing the uterus against it. And in this manner we may rationally and satisfactorily account for the os tinæ being in these cases always remote, sometimes inaccessible to the finger, and constantly behind the ossa pubis.

From this period she progressed in the ordinary manner of pregnancy—she became larger—the motion of the child was strong and frequent, but was felt “on each side, and at her back bone.” On this latter circumstance Dr. M. seems to lay some stress, for he says, “from this it may be inferred that the uterus did not *fully* acquire its natural position in the pelvis; and that the sensations of the mother, when the child moved, seemed to prove that it still remained retroverted.” Now, it is not asserted by Mr. K. that the uterus had acquired entirely its natural situation, but “pretty much its natural situation,” which is sufficient to prove, it was not in a state of retroversion, at the time indicated, and this is all that is necessary to contend for. As regards the stirrings of the child, it could easily be shown that a similar account is given in cases of decided extra-uterine conception, and we should claim it as a mark of such, did we feel we needed such support. Besides, the facts pointed out by Mrs. F. for the motion



of her child, would go no way to prove a retroversion, since in this case the motion would be confined to the sacrum, and Mrs. F. complained it was at each side and at her back bone, the parts that would necessarily receive the sensation in cases of extra-uterine pregnancy.

For a month after the beginning of January, 1802, the period at which the time of gestation would be completed, she was occasionally seized with violent pains resembling labour; these would intermit from time to time; Mr. K. did not examine his patient. But "after this," says Mr. K. "I visited her daily; she continued languid and unwell, and on the fourth day she had shiverings, succeeded with some feverish heat. Her breasts began filling with milk, and by no means in a small degree, for on the fifth day they were as powerfully distended as one usually meets with in a healthy woman who had been delivered that time. All these symptoms were more than sufficient to make me suspect, what upon examination evidently appeared to be the case, *that the fœtus was extra-uterine.*" From this history of symptoms, we are led to conclude, that the death of the fœtus took place with their commencement; and it would prepare us to anticipate the situation of the child as it did Mr. K. and would prevent surprise, that an examination should prove the "fœtus to be extra-uterine." Dr. M. conceals this declaration of Mr. K. as it is a positive avowal of his opinion, and he most certainly had the best opportunity to judge.

"Upon the first and every other examination," says Mr. K. "I found the parts somewhat in confusion,\* the child plainly to be felt through the vagina, the uterus not enlarged, but forced upwards and forward, the os tinæ quite closed; from this last day of pain, all motion of the child ceased. A month after, she became regular in her

\* "Great must be the confusion," says Dr. King, p. 70, "to an accoucheur, when the os uteri is quite closed, and pressed upwards and forward, and not enlarged with labour pains, and the child plainly to be felt through the vagina; let the reader compare this epitome with my paper, and I think he need not hesitate to decide, that this was also a case of ventral gestation."

female health, and has without interruption continued so to this time."

About fifteen months after this, her health began to suffer much—entirely deprived of appetite; night sweats; menstruation ceased; with a number of other alarming symptoms, together with "a sudden discharge of a large quantity of very putrid slimy matter, from the anus," which continued at intervals for a month or more—and after a lapse of time, many bones were discharged from the same part.

Upon this case Dr. M. further remarks, "enough is told to satisfy the reader, that the uterus had been in a retroverted state, occasioning a suppression of urine; that after the suppression was removed, the uterus had not completely regained its proper situation; that it remained in an untoward position till the termination of the full period of utero-gestation, and that the pains being inadequate to the restoring it to its natural state, the parietes of the womb had, either from a laceration or from ulceration, given way, and allowed the escape of the fœtus into the hollow of the pelvis, between the vagina and rectum."

Upon these observations and Mr. K's history we shall offer the following additional remarks:—First. Should it be admitted that the uterus was in a state of retroversion in the earlier part of pregnancy, it is evident, from the whole account we have of this case, it was so in an unimpregnated state (as we have already remarked) and that it was suddenly restored to "nearly its natural position," agreeably to Mr. K's statement. Secondly. That after the cessation of the last pains an interval of but four days elapsed when she was attacked with symptoms which clearly expressed the death of the fœtus; and that, upon examination, it could be distinctly felt through the vagina, a circumstance which could not have obtained, had it been enclosed within the uterus; the uterus not found enlarged but "forced upwards and forward," the os tincæ quite closed. Thirdly. That if the fœtus had changed place by a laceration of the parietes of the uterus, we should have had the

train of symptoms which never fail to follow it; but not one of these were present. The only symptoms recorded at this period were those consequent upon the death of the fœtus, and the secretion of the milk. Fourthly. That if the fœtus changed its situation by "ulceration," it must have happened in the course of four days, a period, Dr. M. himself would admit, far too short for this process; besides, it was not accompanied by a single symptom which would declare it about to take place. Fifthly. The uterus, in this case, could not have suffered from either of these processes, as we are distinctly told, the patient menstruated regularly after a month, and continued to do so until the period of Mr. K's writing the first part of this account, an interval of fourteen months. Sixthly. That the symptoms which preceded the ulceration which permitted the fœtus to escape *ex ano*, were precisely such as always attend this process in these parts, and bear not the slightest resemblance to those which presented themselves at the period Dr. M. supposes the fœtus had escaped from the uterus by a similar process. Seventhly. The discharge which preceded the expulsion of the bones was exactly what would be expected on the rupture of a sac containing an extra-uterine fœtus, and which, perhaps, could not have happened, had it made its way through the parietes of the uterus, either by a rupture or ulceration of them. From all we have said, we conclude, that this case does not in the smallest degree sustain Dr. M's hypothesis.\*

The next case presented for our consideration is the one related by Mr. Coleman. It is as follows:—Mrs. Cooper, on the 25th December, 1798, sent for her midwife, who gave her assurances of a speedy delivery. She was at this time at the end of her reckoning. On the 7th January, 1799, Mr. Coleman was sent for. She had no pain;† but

\* Dr. King, page 74, observes on this case, "We are obliged in a review of all circumstances of this case, to conclude that Dr. M's induction is not established."

† "According to Dr. M's instances of Mrs. Wilkes and Mrs. F's cases, Mr. Coleman's patient should, in five, six, or in a few days, have manifested fresh

upon "examining her, a globular substance was found very low in the pelvis, which he supposed was the head of the child ; but the os uteri could not be discovered." He staid with her some time, and desired to be sent for should her labour come on. He, not hearing from her, called on her two months after, and found her still undelivered and received from her the following account: "She told him she had felt nothing of the child since Christmas day, but was certain she had previous to that time, though different from her sensations on former occasions." "The body," says Mr. C. "had nearly the same appearance as in natural pregnancy, with an unevenness above the os pubis. The whole had not exactly the usual globular form of the impregnated uterus." "I found the child's head," continues Mr. C. "pressing down very low, and could not discover the os tincæ in its usual situation, but thought I discovered it above the os pubis. On endeavouring to pass the finger towards the sacrum, it could not pass, owing to the vagina obstructing it in every direction backwards. I could pass the finger very high by the pubis, in which situation I found the os tincæ as before described." "I examined her again, and concluded it was an extra-uterine fœtus, lying between the rectum and the womb, pressing the uterus up against, and chiefly above the pubis."

Mr. Rigby now saw her, and had seen her in the earlier part of her pregnancy, but was not fully confirmed it was extra-uterine, though there was something extraordinary in the case. Her health, at the period of four months after her full period, had suffered much, and she was reduced to a most deplorable situation. On examining her now, Mr. C. says, "I found an opening unlike the os uteri, and my finger passed immediately into the head of the child. She had ejected during the night a considerable quantity of fœtid bloody water. She had no pain but what I gave her, as I used some force, pressing upon the inside of the bones of the cranium, and endeavouring to dilate the opening."

signs of labour, and if it were really a case of retroversion, she should have been delivered, even of a dead child,"—*Analysis*, p. 90.



She was at last, by different efforts, delivered of a "fœtus in a highly putrid state. It appeared to be a male child at full time when it died, both from the formation of the bones and the size of the fœtus." "The opening in the vagina, through which the fœtus passed, extended nearly to the neck of the uterus. We could now distinguish the neck of the uterus and the uterus itself by the touch, the finger passing backwards into the large cavity from which the child was extracted.\* There was no doubt of a communication between the bowel and the cavity, as some seeds from a cake eaten the day before came away on Mr. Aldhouse's fingers, with a portion of fæces." After much suffering she was restored to tolerable health.

Dr. M. says, "this case is a valuable addition to the series of facts, by which I hope to prove, that retroversion of the uterus, at the full period of pregnancy is no very uncommon occurrence."

On this case we shall remark first, that we have not a word of information of the situation of Mrs. Cooper previously to Mr. Coleman's visit on the 7th January, 1799; and of course not made acquainted with any fact that would lead to the suspicion or declare that she had suffered a retroversion in the earlier part of her pregnancy. Secondly. That this silence would seem to declare, that this condition of the uterus did not exist, and the more especially, as she had been "visited by Mr. Rigby (whose opinions upon these subjects are much to be respected) in the earlier part of her pregnancy," who though not fully confirmed that it was extra-uterine," yet as far as we can collect from Mr. Coleman's statement, did not intimate a suspicion of its being a case of retroversion at full time, but granted it was one in which there "was something extraordinary." Thirdly. That Mr. C's description of this case precisely corresponds with many we have upon record, where dissection proved them to be original pregnancies of the extra-uterine kind; in which the os tincæ could not be felt at all, or

\* Dr. King thinks the whole of the fœtal mass would have passed through the anus, "had not the midwife injured the vagina."

very imperfectly, and that only behind the ossa pubis—we have already noticed this condition of the uterus in our paper, page 247, to which we must now refer. Fourthly. That it is impossible from the nature of things that this could be a case of retroversion, since Dr. M. admits but two modes in which the fœtus can escape from the uterus, namely: by a rupture or by ulceration;—as respects the first of these modes, there is not a solitary symptom recorded, that could give rise to the suspicion that it had taken place; for from this accident Dr. M. himself allows, “the poor woman would fall a victim, in the course of a few days;” and as regards the second, there is as little room to admit it, as the first; for we are distinctly told, that it was a fœtus at full time, as was proved by the conformation of the bones and of the size of the child itself; and also informed that after its extraction, both “the neck of the uterus and the uterus itself, could be distinguished by the touch.” Now, can it be supposed, that the uterus could “be distinguished,” after it had suffered a lesion from ulceration, so extensive as to allow a fœtus and its appurtenances at full time to pass through it? Fifthly. The “ejection of a considerable quantity of fœtid bloody water,” is precisely what occurs, as we have already noticed, where the fœtus is contained in an extra-uterine sac. From these considerations, we cannot withhold an expression of surprise, that Dr. M. should consider this case as auxiliary to his explanations; for to us it appears decidedly hostile, if not fatal to them.

The next case Dr. M. adduces, is that of Mrs. Mainwaring;\* the history of this case differs so little in essentials from the many we have already detailed, that we shall confine ourselves to such parts only as Dr. M. lays a stress upon—for we cannot but regard it as decidedly extra-uterine. The first is, “the patient complaining of a sense of fulness, and a frequent and strong desire to go to stool, and passing with much difficulty but small quantities of

\* Trans. of a Soc. for Imp. of Med. Knowl. vol. ii. p. 287.

urine." We have already remarked sufficiently at length upon these symptoms, and we need not repeat here; the fact is too notorious that these symptoms do not constitute a retroversion, and that they are frequently induced by a prolapsus uteri. The second is, "that when she had attained between the second and third month of her pregnancy, she had strong symptoms of being about to miscarry, but no ovum came away"—in several of the histories of extra-uterine pregnancies, this circumstance is particularly noticed; so that it does not appear, that pain or even a considerable discharge of blood from the uterus, prove the presence of an ovum within its cavity. The third is, "that a tumour in each groin, and which extended almost as high as the navel, were discovered—they were painful upon the slightest pressure." But are tumours in the groins symptoms of a retroverted uterus; we know of no authority for this. These tumours we should rather believe would make against the idea of retroversion, since we are told that one of them "reached almost as high as the navel," and this must have been before the fifth month of pregnancy—now, had this been a retroversion, no such thing could exist, for at the period indicated, the uterus would necessarily be entirely confined to the pelvic cavity; whereas in this case the tumour was higher than in an uterine pregnancy. The fourth is, that "a coffee coloured fluid was now discharged from the urinary bladder, in quantity nearly amounting to three pints in twenty-four hours." But "before this period the quantity was less." Dr. M. wishes to show this to be a symptom of retroversion, by supposing that "such symptoms would very probably be present in a case of retroversion, where the flow of urine was considerably impeded by the uterine tumour, but not entirely suppressed." But unfortunately for this conjecture, it is stated, that "a quantity amounting to three pints was discharged, (though before that precise period there was less,) in the course of twenty-four hours." Now, will any body declare, the flow of urine to be considerably impeded, where at least a quart per diem is discharged? At all events it would prevent

the bladder from sustaining any injury from distention, and that is all that is necessary to contend for in this instance, as it is on this principle Dr. M. would account for this high-coloured urine. As regards this we may remark, that the rupture of a small vessel within the bladder, a circumstance not unlikely to happen, would satisfactorily explain this appearance.\*

Let us now advert," says Dr. M. "to what was discovered by an examination per vaginam, at about the sixth month of her pregnancy."

"A tumour was found in the hollow of the sacrum, occupying its whole extent, and projecting so much forward as nearly to fill the cavity of the pelvis. It seemed to lie between the vagina and rectum, and was less than two inches within the pelvis, reckoning from the external orifice."

"The os uteri was altered in its shape and situation, being pressed against the bladder and pubes. The cervix uteri was so fixed in its situation, as to resist any attempt which was made to move it upwards. From these circumstances, the urine was passed with difficulty."

"The shape of the tumour in the hollow of the sacrum was nearly round, but somewhat flattened upon the anterior part. In breadth, it was supposed to be between three and four inches, and in thickness from two to three. It felt moderately firm."

On the portions of Mrs. Mainwaring's history, which Dr. M. felt it important to quote particularly, we shall only remark, that we have already said enough on the presence of a tumour in the pelvis, as above described, with all its consequences, to prove its total insufficiency to constitute a retroversion. And, that Mr. M. himself has declared, "that the tumour in the pelvis was lower than that which is found in cases of retroverted uterus; and that the posterior part of the vagina was without the puckering occasioned in that disease by the fundus falling down behind the vagina." On these observations, however, of Mr. M.

\* Dr. King declares he has known this to happen in several instances, p. 100 of Analysis, &c.



Dr. M. remarks, that they cannot be looked upon as proofs that the tumour was not occasioned by the retroversion of the womb." "For," continues he, "the situation of the tumour in the vagina, described before in Mrs. F's case, was much lower than ordinary in retroversion." But what does this prove? Certainly nothing, since Dr. M. has not satisfactorily shown Mrs. F's to be a case in point.

"After a long series of sufferings, this poor woman's case terminated favourably, by the discharge of the bones, &c. of a fœtus, through the rectum." And it was found upon examination, that "the projecting part of the cervix uteri into the vagina (about eighteen months after), was shorter upon the left side than upon the right. The vagina might be said to be somewhat constricted upon the left side, so that the cervix uteri was less moveable there than upon the right, and some pain was felt in touching this part with the finger." "Now this," says Dr. M. "I think is an additional proof, that the fœtus was at first contained within the womb, which being pressed down in a retroverted state between the vagina and rectum, and unable to extricate itself from that position, underwent the process of inflammation, adhesion and suppuration, and thus was relieved of its contents; but was consequently diminished in size."

On these concluding remarks of Dr. M. we shall briefly observe, that all the objections we have already urged against either rupture or ulceration of the womb in these cases, are in full force against them; and we shall rely upon our former arguments to prove Mr. Coleman's to be a case of extra-uterine conception; and that the contraction on the left side of the os tinæ can satisfactorily be accounted for, in the healing of the wound in the vagina, without the smallest necessity of supposing the health of this part to be involved in the destruction occasioned by ulceration.\*

Having given our reasons for dissenting from Dr. M's

\* "I must, therefore," says Dr. King, "dissent from Dr. M's inference, and coincide with Mr. Coleman, that this was an indubitable case of ventral gestation."—p. 97.

facts and inferences, we shall conclude by stating what has been proposed and effected for the relief of the woman who may be the subject of an extra-uterine conception. They may be divided into two general heads, viz. First. To attempt the liberation of the fœtus by an operation; or, Secondly. To exclusively trust to the powers of nature. As far as we have been able to ascertain, no very correct or decided opinion upon the subject of an operation to relieve an extra-uterine fœtus was entertained previously to the time of Dr. Charles Kelly,\* who, having witnessed the death of a patient under these circumstances, in the year 1756, declares that in his opinion "the only method that would promise success in such a case, is to make an incision through the posterior part of the vagina, against which the head is strongly pressed, and which, in consequence of that pressure, becomes very thin. The child may, by such means be easily extracted." Since that time this operation has been several times performed with varied success:—by Lauverjat,† with success to the mother; by De Lisle,‡ with success to the child;§ by Novara,|| with success to the child;¶ by Dr. King,\*\* with entire success to both.

The method originally proposed by Dr. Kelly, and adopted by Dr. King, appears to have decided advantages over the method pursued by Novara, and recommended by others, whenever the child is forced into the pelvis, and can be felt through the vagina or rectum. It is then a simple operation, requiring only an incision through the coats of the vagina, or the part most tense or protruded.

\* Med. Obs. and Inq. vol. iii.

† Sabatier Med. Operat. tom. i. p. 316.

‡ Journal des Science, tom. x. p. 384.

§ That is, the child was born alive, though it survived its birth but three quarters of an hour. It was premature, and supposed to be between the sixth and seventh months.

|| Ibid. tom. iii. p. 119.

¶ This was, however, by gastrotomy. The mother died on the thirty-third day after the operation, and her death rather attributable to the delay than to it.

\*\* Analysis, &c.

By this operation the risk of peritonæal inflammation is much diminished, and the wound itself no way so serious. We cannot but recommend the reading of his Analysis to every medical practitioner, or at all events the case itself as published in the Medical Repository of New York. But it should be observed, and it is judiciously insisted upon by Dr. King, that the operation should be as speedily performed as the nature of the case will permit; for by an early operation, both mother and child may be preserved.

As regards the second mode, we have but little to say; for as we believe Dr. M. to have erred in his premises, we cannot entertain much respect for his deductions; and were we even to grant the legitimacy of both, we should unhesitatingly reject them for ourselves; for to us it really appears but a plan to see how much poor human nature can bear before aid would be considered justifiable.

Dr. M.\* considers the subject of extra-uterine conception further; but as his speculations upon this subject, are not necessarily connected with our present object, we shall not notice them now, but shall reserve the right to animadvert upon them separate and apart, at a future time.

\* P. 66.

A CASE OF

**SUPPRESSION OF THE MENSES,**

ATTENDED WITH UNUSUAL CIRCUMSTANCES, RELIEVED BY THE  
VOLATILE TINCTURE OF GUAIACUM.

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ON the 6th of June, 1820, I was requested to see the daughter of Mr. D. F. who was represented to be suddenly taken extremely ill. I immediately visited her, and found her sitting up in bed, with her head tightly bound by an handkerchief. Her mother informed me, she was attacked with a severe pain about the region of the uterus, and almost instantly after with delirium or mental alienation. She complained much of her head, palpitation of the heart, and occasionally of a sense of suffocation. Her mental alienation was not constant; but, on the contrary, she had frequent lucid intervals, after a short continuance of the delirium, and in this latter condition, she would manifest extreme affright, begging protection from those around her, against some supposed impending and terrible danger.

Her situation previously to the severe attack just mentioned, was thus related by her mother:—"She had not menstruated for seven or eight months;" (but it was afterwards determined, by the patient herself and sister, that she had not for more than a year) "that her belly swelled very much—her breasts became enlarged—she had nausea and vomitings in the mornings," &c. In a word, she had all the usual signs of pregnancy.



Examining the abdomen carefully, I found it considerably distended ; there was a circumscribed tumour within it, which I was very certain was an enlarged uterus. While conducting this examination, I thought I distinctly perceived the motion of a fœtus.

I merely prescribed for the more immediate and pressing symptoms. As there was fever, she was ordered to be bled, purged, and to observe an abstemious diet. By these means she was in a few days, apparently, perfectly well, and so remained for nearly a fortnight. At the end of this time she was again attacked with precisely the same symptoms as just described, and which were subdued by the same remedies. These going off she again enjoyed an interval of two or three weeks of health, and was then seized as before, and a third time relieved by the same routine.

The abdomen continued to augment in size, and the feet and ankles were also swoln. I again carefully examined the abdomen, and now found an evident fluctuation. I had formerly thought I had perceived this, but it was obscure, and as I was disposed, in spite of myself, to suspect there was a gravid uterus, and imagined I might have been mistaken with respect to the fluctuation, though the young lady's character was, from the respectability of her connections, and the propriety of her own conduct, beyond suspicion. It was, however, no longer doubtful, that there was water within the cavity of the abdomen, and also that the uterus, from some cause or other, was much distended. I began to reflect seriously upon the case, and to weigh with great caution, all the circumstances connected with it. The young lady's character was without reproach—she had now been obstructed more than thirteen months—there was no perceptible increase of size in the uterus since I first examined her, though nearly two months had elapsed. Had she been pregnant at that time, she should now have been delivered, for the fundus of the uterus, if it was the uterus I first felt, indicated a period beyond the seventh month, for it was half way between the umbilicus and the scrobiculus cordis. When I reflected upon all these circumstances, I

thought the complaint should be treated as ascites, and I therefore prescribed the volatile tincture of guaiacum, in doses of a tea-spoonful every morning, noon, and evening, in a wine-glass full of milk.\* After taking this medicine for a few days, it purged her very briskly, and made her discharge very large quantities of urine. The tincture thus affecting her bowels, a few drops of laudanum were added to each dose of it, which enabled her to persevere in its use for three weeks without further inconvenience: at the end of this time, not a vestige of water was discoverable in the abdomen, but a serous discharge was observed from the vagina, which was soon followed by a sudden gush of fluid blood, to the amount of about three pints, which soon abated in quantity, and at the end of a week entirely ceased. She menstruated at regular periods after this, and now enjoys perfectly good health.

The above case is remarkable for the condition of the uterus, and the dropsy which accompanied it. This organ it would seem, did not refuse its habitual office, but that from some cause or other, the os uteri became closed so as to prevent the escape of the menstruous fluid. How this was effected is left to conjecture. May not the secretion natural to this part have become vitiated, so as to have its tenacity much increased, and thus block up the uterine outlet? That it was owing to some such cause is rendered probable, by the change just mentioned, namely, of the return of the ordinary secretion of the parts, in more considerable quantity than usual, which, after continuing a short time, was followed by a gush of fluid blood from the uterus.

\* It may not be amiss to observe, that the preparation of Guaiacum which I employ, differs from that commonly obtained from the shops. I shall therefore subjoin my formula.

R. Pulv. Gum. Guaiac.	eight ounces.
Carbon. Sod. vel potas.	three drachms.
Pulv. Piment.	two ounces.
Alcohol. dilut.	two pounds.

Digest for two or three days.

The volatile spirit of ammonia is added *pro re nata*, in the proportion of one drachm to every four ounces of the tincture.

Two causes might operate to the relief in this case. The natural secretion being restored, would, of course, diminish the force with which the obstructing cause might adhere to the sides of the neck of the uterus, or the uterus being much distended, would, as in common pregnancy, begin to react, and this with sufficient force to overcome the resistance offered at the orifice of the uterus, now perhaps weakened by the restoration of the natural and healthy powers of this part. This conjecture is rendered more than probable, by the very sudden way in which the relief was obtained. At all events, it could not have arisen from any deranged condition of structure in the neck of the uterus, as this most probably would not have admitted of cure.

This case is also interesting in other points of view. It marks with great force the sympathies depending upon the uterus, when its cavity is in any way occupied; for in the case just related, all the rational signs of pregnancy were present, without impregnation—and to excite the sympathies, it would seem to be only necessary to have the parietes of the uterus put upon the stretch: and again, that the secretion of the menstruous fluid will not interrupt the play of sympathies between the womb, stomach, and breasts, provided there be any cause resident in the uterus capable of distending its sides. It also proves the decided efficacy of the tinct. guaiac. in removing dropsy and amenorrhœa. In a moral point of view, it is not less interesting, as it shows how easily a derangement in the uterine economy may lead to the most cruel and unjust imputations, even to affect life itself. Of the latter kind, Foderé\* gives us an affecting account. “A young woman had her menses suddenly suppressed in consequence of a fright, and sought every aid to restore them, without effect: she was at length married with a view to provoke a return, which succeeded after a time, and she discharged a great quantity of fœtid matters. This fact was proved by the husband and the

\* Med. Legal. vol. i. p. 476.

medical attendants. It so happened just at this period, that two children were found exposed and destroyed by cold—a suspicion fell upon this young woman because she was known to have had an enlarged abdomen, which very suddenly had subsided. The judges of the district, not free from prejudice against her, appointed a month after, a physician, a surgeon, and two midwives, to examine her, and determine, whether she was delivered of a child, or relieved of a dropsy of the uterus—who reported, “that they had discovered marks of delivery.” In consequence of this, the poor woman was condemned to death, for concealing her pregnancy and her children. An appeal however was made to parliament—and in consequence of two consultations, held by several physicians and surgeons of the greatest eminence, she was acquitted.



AN ESSAY ON

UTERINE HÆMORRHAGE.

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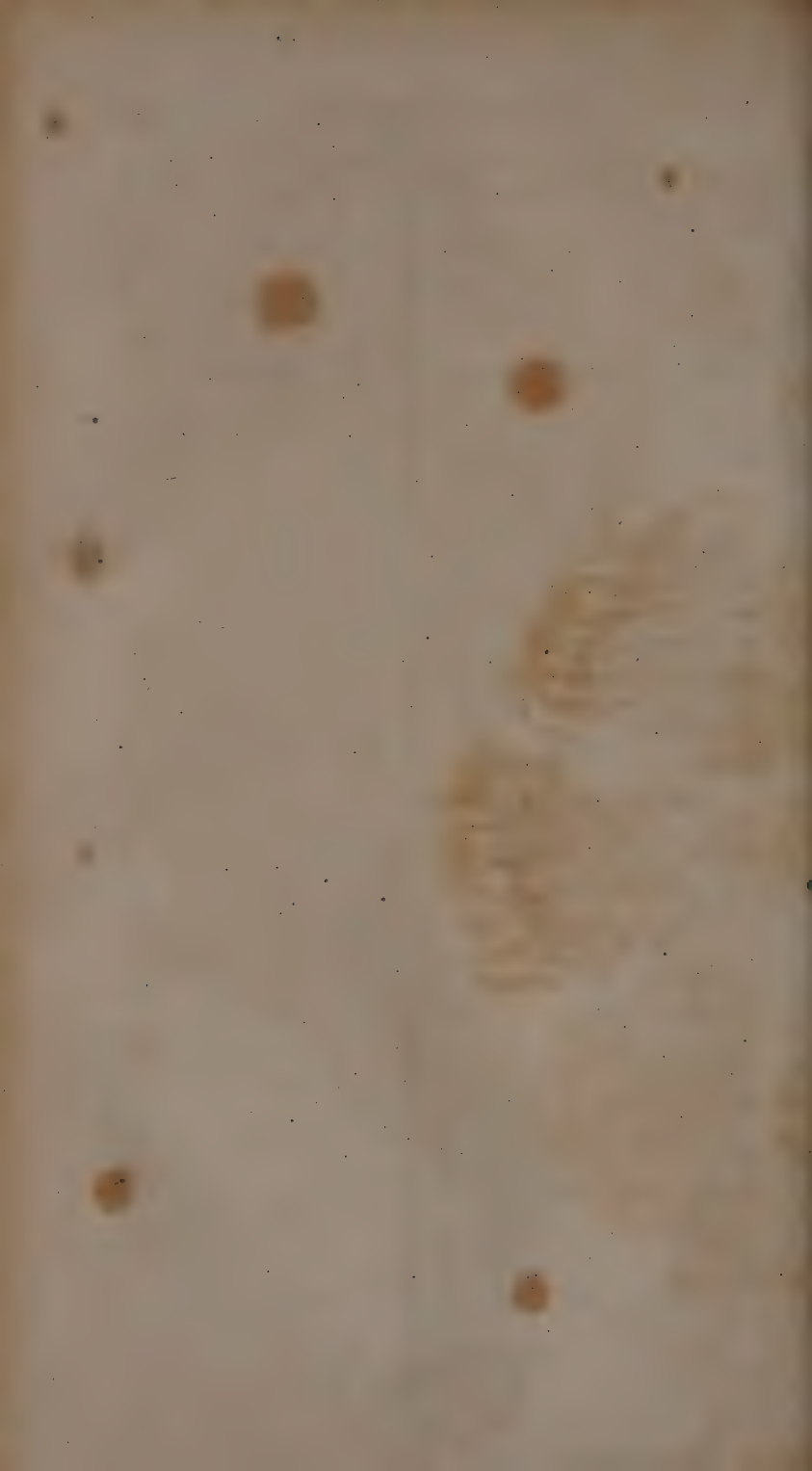
THE frequency of occurrence, and the dangerous tendency of uterine hæmorrhage, render it peculiarly interesting to the medical practitioner. And this interest is increased when he considers the discordant opinions entertained of its origin, and but in too many instances the inefficacious modes proposed for its treatment. With a view to reconcile the first, and to point out for the second a more decided plan of cure, the present observations are offered.

In the earlier writers upon this subject, we find considerable diversity of sentiment; not only with respect to the source of the disease, but also as to the mode of treatment. While some were of opinion that the neck of the uterus and a portion of the vagina furnished the expended blood, wherever abortion or miscarriage did not take place;\* others contended, the discharge could only happen from a separated portion of the placenta or membranes.† A few were content to rely upon the efforts of nature, and thought it might be even mischievous to interrupt it—or at farthest depended upon temporising applications to the vagina, or upon the exhibition of some inadequate astringent by the mouth—while, a much greater number insisted it was a most serious accident, and that the woman's safety exclusively depended upon the expulsion of the ovum, or the

\* Mauriceau, La Motte, De Graaf, &c.

† Puzos, Pasta, Kok.





premature delivery of the child—from which it would appear how vague and uncertain their notions upon this subject were.

We are not, however, to include in this censure, the later writers upon midwifery. From them we confess to have received much important information, and to be indebted *collectively* for nearly all we know of its mode of treatment—though we must at the same time declare, we do not regard any *one* of them as having brought into view all that we consider valuable, or necessary to be known, upon this formidable complaint.

In prosecuting the inquiry into this subject, we do not feel ourselves bound, to give a detailed account of the notions entertained by every writer within our reach, and shall merely pledge ourselves to the faithful selection of such opinions and observations as may in our estimation merit most consideration. In doing this, we hope we are performing an acceptable service to those whose leisure or opportunities will not permit them to do it for themselves; and sincerely trust we shall be forgiven when we are found to depart from high authority or generally received opinions, which do not comport with our own views or experience. For the latter, we can with much honesty declare, it has been rather ample than confined.

The mode of inquiry we propose to pursue is—

First. To consider very briefly the nature of the connection of the ovum with the internal face of the uterus.

Secondly. To investigate the causes which may impair this connection, and thus expose the source from which the blood is derived.

Thirdly. To examine into the mode of action of these agents in effecting this lesion.

Fourthly. To point out the several periods of uterogestation at which this may take place—and trace the various consequences which may result from these periods.

Fifthly. To notice the mode of treatment under the different stages and circumstances, which may accompany the disease.



I. *The connection of the Ovum with the Uterus.*

Soon after the ovum is deposited within the cavity of the uterus, we find it connected through its whole extent of surface, with the internal face of this organ. Both uterus and ovum contribute to this end—on the part of the womb, we find it produce a soft spongy substance called decidua—on the part of the ovum we discover its external covering or chorion shooting out innumerable vascular fibres—and both when united serve as the bond of union between ovum and uterus. The efflorescence on the uterine surface, like that which covers the ovum is decidedly vascular—and it would seem, that these minute vessels, interlock with each other after a certain period, so firmly, that they cannot be well separated without rupture.\*

It is not necessary to our present purpose to inquire in what manner these vessels subserve the purposes of fœtal growth—we only clearly understand, that when the integrity of either set be injured, there will follow a discharge of blood, proportionate to the extent of injury—the part of the uterus at which it may happen, and the advancement of gestation. Should a large portion of the ovum be detached in the earlier months, the quantity of blood that may issue will be commensurate with that sur-

\* Mr. Burns, (Principles of Midwifery, p. 181. Ed. 2.) who is high authority, is of opinion, that a separation of the maternal and fœtal portions may take place—this may be, though not very susceptible of proof, nor is it perhaps of any great practical importance, yet if it be a fact, it should be recorded as such. He says, “at times the fœtal and maternal portions separate, and the first is expelled before the second, forming a very beautiful preparation.” We have seen several ova of the kind we believe Mr. B. alludes to, but their appearance would lead us to a very different conclusion. But for fear we may misunderstand him, we will describe what we have seen more particularly. In several instances we have known ova expelled from the uterus, after a considerable continuance of pretty severe efforts, which were decidedly without a vestige of the uterine product attached to them—they were evidently, and indisputably, examples of the ovum being surrounded or covered by the entire product of the chorion, without their having formed any union with the decidua—their discharge was not attended by hæmorrhage, though there was slight discharges of blood. We own a very beautiful preparation of this kind at this moment.

face, especially if it be from the body or fundus. If the separation take place near the neck, the discharge will not perhaps be so abundant, as this part is considered to be less vascular than the other portions of this viscus.

But the latter of these circumstances will be influenced by the period of gestation. As a general rule it may be said, that the quantity of blood which may be expended will be in proportion to the advancement of pregnancy.

## II. *The causes which may tend to destroy this connection.*

In consulting authors upon this subject, we shall find that a variety of causes are enumerated as capable of destroying, to a greater or less extent, the connection between the placenta and uterus—and it is agreed by far the greater number, that no considerable hæmorrhage can occur unless this happen. By a few it was believed that a mere separation of the membranes was sufficient for this purpose : but of this we have no good evidence—and should it be even true, it can only refer to this accident after the fifth month—for until this time, the whole uterus may be considered as being lined with placenta. Mauriceau, La Motte, and others, supposed that a discharge of blood during pregnancy must necessarily produce abortion, if it proceeded from the proper cavity of the uterus, as this could only happen by a destruction of continuity between the ovum and uterus : while Dionis thought it could take place without such a consequence ; as he imagined the placenta occupied the fundus of the uterus alone, and that those vessels which furnish the menstrual discharge before impregnation, might also yield it during gestation, if they were in a plethoric state. De Graaf was of nearly the same opinion. It would be a waste of time to labour this point farther at this place. We shall only say, that we agree with those who derive the blood expended in flooding from the exposed surface of the uterus, by a portion or the whole of the placenta separating from it.

In enumerating the remote causes of hæmorrhage, we

shall only name such as are most generally believed capable of this effect. 1st. A too short funis: 2d. Mechanical violences: 3d. Passions or emotions of the mind: 4th. Plethora before delivery: and after delivery. 1st. Atony: 2d. Spasm: 3d. Humoral engorgement: 4th. Unequal contraction of the uterus: 5th. Inversion.

Though all these causes have been assigned for the disease we are considering—still it is sufficiently difficult of explanation how some of them act to produce it. When violence of any kind is offered a pregnant woman, and she miscarry, or is prematurely delivered, the cause, from its force or extent at first sight, appears capable of the end, and there all investigation ceases. It may not therefore be time ill spent, to inquire into their respective agencies.

### III. *Mode of action of certain of the remote causes.*

And first of too short a cord. It was the opinion of La Motte, that the cord may be naturally or accidentally too short—in which case it might be the cause of hæmorrhage. He gives a case illustrative of this assumption—but confesses it was the first and only one he ever met with. The bleeding proceeded from one of the umbilical vessels, at a portion which was folded into a kind of knot, which part yielded from the accidental shortness of the funis. Levret met with a similar instance.\* It must however be confessed by all conversant with the practice of midwifery, that though this may be a cause of hæmorrhage, it must be a very rare one—or the extensive practice of these three celebrated authors would have furnished more examples.

It is not at all extraordinary that we should have only a few cases of this kind upon record, since we do not well perceive how it can take place. Though the cord may be very short, either naturally or accidentally, still there must be great difficulty in breaking it by any effort of the child; for if the waters be preserved, the specific gravity of the

\* Baudelocque also mentions a remarkable case of the kind. *Midwifery*, par. 1084.

child and them will so nearly correspond, that the weight of the child may be considered as almost nothing—so that whenever the cord was put upon the stretch, the child would instantly move towards the force, and thus destroy its influence. If, on the other hand, the uterus should be emptied of the waters, it would instantly almost embrace the body of the child so firmly by virtue of its tonic contraction, as to render it almost immoveable, and consequently it could not exert so much force as to injure the continuity of the cord. We may then safely conclude, that if it take place, it must be attended by such a combination of circumstances, as will always render it of extremely rare occurrence.

Another inconvenience is said to arise from too short a cord—namely, a separation of a portion or even the whole of the placenta during labour. Leroux says “that the placenta may be separated entirely or in part in consequence of too short a cord. This case,” he says, “is met with in practice, and he is persuaded that the greater part of the floodings which happen during labour after the escape of the waters, and when the head is in the lower strait and the pains are almost useless, has no other cause.”\* We were not a little surprised at this declaration, as we did not recollect a single instance, nor could we find it among our notes, where the hæmorrhage was attributed to this cause. And we are firmly of opinion, that whenever too great a shortness of cord shall become a cause of flooding before delivery, there must exist, at the same time, a preternatural feebleness of union between the placenta and uterus: for if the usual degree of adhesion obtain, the cord would break before the placenta would separate, as the force which it would exert upon this mass would be at right angles with its surface, and would require a much greater power to separate it, than could possibly be employed by any movement of the child, and more especially at the time indicated, namely, after the discharge of the waters.

\* Pertes de sang, par. 162.



Besides, were it possible that this cause could produce a separation, it would most probably be opposite to the point of insertion of the cord, which, generally speaking, is near the centre of the placenta: if it effected it here, it would almost certainly be concealed, as the surrounding attachment would act as a dyke to the influent blood, and thus conceal the injury at least until after the birth of the child. But Leroux himself confesses the discharge is not great, and is more dangerous to the child than to the mother—and also that he has received many children which did well, notwithstanding the cord did not exceed in length six inches. Many authors mention this uncommon shortness of cord, and mention it in such a manner as to lead to the conclusion that it is more frequent than is imagined. It may be so—though we have never in a single instance met with a funis, the natural length of which did not nearly double this.

We grant a too short cord may be extremely inconvenient, and create considerable embarrassment at times, especially after the head is protruded through the external parts: at this time all the accidents stated above may happen and can only happen then.

2dly. Mechanical violence: 3dly. Passions or emotions of the mind: 4thly. Plethora. Each of these causes may produce uterine hæmorrhage, and they all perhaps have in their turn done so. However, the mode in which they effect this, is not so well understood as it may deserve—the whole of these causes have one operation in common upon the system—they all induce an increased force of circulation, and which is generally considered sufficient under certain circumstances to produce the evil in question. It has been thought, that whatever gave an increase of force or velocity to the circulatory system of the mother, must almost necessarily, in consequence of the large size of the hypogastric and spermatic arteries, the short distance they have to travel before they arrive at the uterus, together with their great increase in that viscus as gestation advances, very much affect the condition of the ovum within

its cavity—that the arterial vis a tergo must act mechanically upon the ovum, and by mere force of circulation drive it from its connection with the uterus—that plethora must act pretty much after the same manner—and as a proof of this, it is said, that the periods at which the menses are wont to return, are those at which abortion is most readily provoked; for at these times, though the uterus is impregnated and this discharge has ceased, still the blood is sent in greater abundance than usual, until the demands of the embryo are such as to employ it, without suffering the vessels to become engorged.

But those who reasoned in this manner did not seem to have a very clear idea of the nature of the union between the ovum and the uterus, since they differed as to the mode. While some insisted that the blood was transmitted *pleno rivo* by continuation of canal from the mother to the placenta, others did not think this necessary, as mere turgescency within that mass was all sufficient for the end proposed. Though we do not mean to deny altogether the influence of an increased circulation—we are disposed to very much limit its agency in producing a separation of the ovum, either in part or entirely from the uterus. For were a mere increase of circulation all that is required to effect this end, no woman should escape aborting who may labour under high arterial action—thus fevers of all kinds should be followed by this accident, which is contrary to all experience. We are obliged then to suppose something more necessary than an invigorated circulation, to produce this effect.

We might indeed insist, that nature has attempted with some success to guard against this contingency, by the peculiar construction of the uterus itself. In the early months there is comparatively but a small quantity of blood sent to the uterus, because the necessity for it is comparatively small—and the force of even this is diminished, by its passing through vessels of small size, and much folded, or convoluted.\* This provision is highly important to the

\* Baudelocque, &c.

welfare of the ovum at this period, since its connection is not so well established as it afterwards becomes, as gestation progresses. The liability therefore to abortion is greater in the early, than in the later stages of pregnancy—for as the union between the chorion and decidua is not yet well confirmed—as the attachment of the latter to the internal face of the uterus is proportionably slight—and as the extent of surface which the ovum now presents is very small to that which it offers in the more advanced states of pregnancy, and as it can of course be affected by smaller causes, it will be seen that a separation will be more easily induced, and prove much more injurious to the well being of the embryo, than a larger one at another stage.

In the more advanced periods of utero-gestation, the circulation becomes freer, and the vessels pretty rapidly increase in size.\* Yet, as we have just intimated, the woman is not so liable to the accident we are considering—now, were nothing more required to induce hæmorrhage than an increase of circulation, why should it not more readily occur at this time, than earlier? since it must be admitted, that more blood is now sent, because more is required—that the vessels are much larger—and arterial action increased in the exact ratio of their augmentation. To comprehend this, we must advert to another part of the uterine economy, in which nature appears to have been studious of the safety of the ovum, by a new provision in organisation. Thus however much the vessels of the uterus may have augmented in size, those which directly administer to the necessities of the fœtus, do not alter in the same proportion. There is every reason to believe that the relative sizes of these two sets of vessels bear a much greater relation to each other in the early, than in the later months—so that the risk of injury from an impetuous circulation is diminished instead of being increased.

It must however be understood, that a given space of exposed uterine surface, will yield blood (*cæteris paribus*) in

\* Baudelocque, &c.

proportion to the advancement of gestation, because the vessels which furnish it have increased, in proportion to this advancement. Now should the deciduous portion of this viscus be removed, it would necessarily expose the extremities of those vessels which yield a supply to an infinity of others, which terminate in, and in part constitute the placenta.

We know of no one who has clearly explained the manner in which the blood is conveyed into the minute vessels which constitute the decidua. That there is however, a peculiar arrangement for this purpose is certain, because there is an absolute necessity for it, since, were the blood conveyed to the ovum *pleno rivo* by vessels of the same size as those which furnish it from the proper substance of the uterus, or even of much smaller capacity, but subject to the same impulse, it follows, that it would be liable to injury from every increase of arterial action, which as we have attempted to prove, is not the case.

Besides, injections prove that a portion of the decidua can be completely filled—and that it consists of infinite congeries of vessels, whose respective size bears no proportion to those terminating immediately upon the internal face of the uterus, or those which are directly interested in conveying blood to the ovum.

Is it not more than probable, then, that each vessel which may terminate in the uterine cavity, has a great number of very fine ones corresponding with it, and which constitute in part the decidua? Is this not the mode which nature has adopted, to prevent the evils which must necessarily result from a hurried circulation? Is this not partly proved by the fact, that when the placenta is removed, and the uterus does not contract, that we have an overwhelming flooding? And may we not add, that such a contrivance is essentially necessary to the well being of the ovum, as well as to the security of the woman after she has expelled it? for were it otherwise, we should always have a rupture of vessels upon the separation of the ovum, or upon the casting off of the placenta from the



uterus—but agreeably to this scheme, we have only an exposure of their extremities, which the contracting uterus almost immediately shuts up.

The decidua then performs two most important offices in the economy of gestation : first, by its great vascularity, as we have just pointed out ; and secondly, by its sponginess and compressibility, which arise from the disposition of its vessels. We trust we advance no absurdity when we say, that most probably one of the uses of the decidua being so cellular and compressible in the early months of pregnancy, is to obviate consequences which might result to the feebly fixed ovum, were it otherwise, from external violence, or internal impulse. By its interposition and softness, vibration, however excited, would in part be certainly arrested in its progress to the ovum—and in more advanced gestation, the same immunity from risk of this kind would follow, from the peculiarly soft and yielding texture of the placenta—for at this time, injury could only happen from a separation of one of its portions—disunion of the membranes yielding little or no blood.

From what has been said, we think we have rendered it probable, that something more is required than an increased force of circulation to effect a separation of the ovum in the early months—or of the placenta in the more advanced periods of pregnancy—and that something we believe to be uterine contraction—as without this we are at a loss to understand the *modus agendi* of the remote causes. We shall not pretend to say how the various causes we have enumerated above induce this action—though we are certain that this effect is produced through their agency, and for the following reasons :—1st. Because, mere circulatory impulse appears from the anatomy of the uterus and ovum to be inadequate to this effect—since neither abortion nor premature delivery follows as a consequence when this condition has been present in its highest degree. 2dly. Because, contraction in every instance is essential to the separation of the placenta, in abortions, premature labour, or delivery at full time. 3dly. Because,

we frequently detect this cause, hours, or sometimes even days, before the eruption of blood; and because, so long as this contraction continues, hæmorrhage will not cease, unless we diminish the bulk of the ovum, or interrupt its return by proper remedies. We are aware that objections may be raised to the reasons just given: it may be said that all testimony is against our first, as we are told by writers from the time of Hippocrates downward, that plethora is frequently a cause of hæmorrhage, and that abortion is often prevented by the loss of a few ounces of blood. Be it so. We also believe such to be the fact. But this is no contradiction, since this condition of the system may act very differently in separating the ovum, than by mere impulse. The vessels in the proper substance of the uterus will and must partake of the general fulness of the system. They are of course distended more than ordinary—in consequence of which they must act as so many wedges to the uterine fibres which, by being thus stimulated, are made to contract.\*

To the second, it may be said, that we have no evidence of this in the cases under consideration. It is true, we have no positive evidence, but we have strong presumption that it is so. Thus, in those instances which fall immediately almost under our inspection, we find that the placental mass is separated only by contraction—for when this does not take place, the after birth retains its adhesion with the uterus—hence, it is always solicited for this very purpose when absent at the termination of labour.

To our third, it may be objected, that in many instances hæmorrhage comes on without being preceded or accompanied by the slightest pain. This, though we also admit, does not prove there has been no contraction of the uterus,

\* We cannot perhaps better illustrate our idea of the connection of the vessels of the decidua with those of the uterus, than by comparing them to fine camel's hair pencils—the quill part to represent the uterine vessel, and the hairy fibres the vessels of the decidua attached to it—the calibre of the quill being equal to the area of the hairy fibres—by this arrangement, the circulating force will be necessarily so much diminished by its almost infinite division, that little injury can be sustained by the ovum, by a mere increase of circulation.

for we well know that pain is not essential to this end—The uterus may and does contract, and sometimes with great violence, without the addition of pain—and which is well illustrated, by what almost always happens after delivery, namely, the spontaneous separation of the placenta as it is termed, in which case, contraction is unaccompanied by pain—and also, by what very uniformly takes place previous to the painful state of labour, the alternate contractions of the uterus, as detected by passing a finger into the os uteri, where it will be found that the membranes are alternately tense and relaxed, and if a hand be applied to the abdomen, the uterine globe will be felt to harden and relax, as contraction may be present or absent. Yet during this time no pain is experienced. Furthermore, by that peculiar contraction of the uterus called the hour-glass contraction—where the placenta is imprisoned in the upper chamber of the uterus by the body contracting very forcibly below—and so firmly does it maintain this condition, that it requires no common force to overcome it—yet there is no pain.

To the fourth it may be observed, that pain, when it attends, is rather a consequence than a cause—for such disturbance has been given to the uterine economy by an increased circulation, as to call in the aid of pain to free itself from the useless burthen, as the ovum has now become, because of its extensive or entire separation from the uterus, and must be considered rather as an extraneous body than a living substance. To this we would answer, that it is sometimes strictly true as regards the ovum, and is an event which always takes place when the embryo or fœtus has lost its life. But does it follow, that because pain, which must be considered only as an evidence of contraction, becomes necessary for the expulsion of the ovum, that it may not have existed before, or that the contractions may not have been often repeated without manifesting themselves by pain? Certainly not. Besides, we know that painful contractions may have accompanied hæmorrhage for a considerable length of time without the ovum

being destroyed, and the woman notwithstanding go her full time. Of this we, as well as others, have seen more than one instance. Yet had these contractions been permitted to have continued, they would inevitably have caused abortion.

Again it may be said, and truly, that uterine contraction may take place, and even of a very powerful kind, without being followed by a separation of the ovum or placenta—Were this not the case, indeed, hæmorrhages and abortions would be much more frequent than they are. But to obviate the force of this objection we may only state the fact, that ova do not always adhere with equal firmness to the uterus, nor does even the placenta itself, after it has become located to a certain portion of the uterus. Of this we have abundant proof in the many instances of habitual aborting, and in those where apparently very slight causes will occasion premature labour or early miscarriage. Some women appear to have ova so ill established, as to require but the slightest mental emotion or corporal exertion to have them cast off. This is notorious to every practitioner.

It may also be alleged, that those cases of hæmorrhage which are accompanied by pain, consequently by contraction, are less dangerous, and of more easy management, than where this does not obtain. Now were contraction necessary to produce this disease, how is it that it can serve to remove it? This presents no difficulty. The whole truth is not told. Where the ovum is about to be cast off either in the early, or later periods of the pregnancy, or where there is no chance of its preservation from the effect already produced upon it, then contraction is useful, as it proves the healthy disposition of the uterus, so far as this circumstance is concerned. By it, the ovum is completely separated—and cast off,—the bleeding put a stop to, and the woman secured from danger. But, let us ask any practitioner of experience, whether he has not uniformly found those cases which have been attended with pain, always of more difficult management, than where none existed. We are sure he will answer yes. Now, if this



be true, and that it is so cannot be doubted, does it not decidedly prove that contraction tends to increase the disunion or maintain the separation, as well as to have produced the original lesion? This fact too is so notorious, that every body who has a view to the security of the ovum, endeavours in the first instance to diminish or destroy uterine contraction by the exhibition of such remedies as may be capable of such effect.

It may not be amiss to inquire how far we may have a control, or whether we have any, over uterine contraction after it has once been called into action. The no small authority of Mr. Burns is against us when we say, we think we have—though confessedly difficult of subjection. Yet, as it is a matter of high consequence to ascertain the truth upon this subject, we hope to be forgiven if we differ from that respectable writer. He says, “when abortion is threatened, the process is very apt to go on to completion, and it is only by interposing, *before the expulsive efforts are begun*, that we can be successful in preventing it; for whenever the muscular contraction is *universally established, marked by regular pains, and attempts to distend the cervix and os uteri*, nothing, I believe, can check the process.”

That it is a matter of uncertainty whether we succeed in our attempts to arrest uterine contraction after it is “established” must be acknowledged. But that it is never attended by success we cannot concede—nor should the principle ever be inculcated, as it paralyses exertion, and withholds from the suffering female that comfort which the attempt rarely fails to give. Our own experience would, we think, in more instances than one declare, that we have been rewarded for the attempt to interrupt uterine contraction—and should it fail nineteen times out of twenty, we are surely not justified in withholding the probable means. We therefore make it an invariable rule to treat the case as if we expected to meet with success—and have had no reason to suspect we are not doing our duty. There is one case however, in which we never interfere with the slightest

prospect of a happy issue—and that is where the process of gestation has unequivocally ceased—and of which we take but one circumstance as absolutely certain, namely, where the breasts have become tender and tumid, and then pretty suddenly subside. It would here be a forlorn hope to administer remedies with a view of retaining the ovum.

We are disposed to believe that this circumstance is the only one which marks the loss of life of the ovum with sufficient certainty; it is perhaps the only one that is unequivocal, since all others may be said to be deceptive. This mark was known to Hippocrates, and has, we believe, ever since his time stood the test of experience. So long then as this sign be absent, we do not relax in our attempts to preserve the ovum. It must however be confessed, that we have known the ovum cast off where this symptom was wanting. Yet we are persuaded in each of these instances that the ovum preserved its vitality almost to the last moment, and that its expulsion was owing to the indomitable nature of the contractions of the uterus—and we think that this has obtained most generally with women who are in the habit of miscarrying. We do not stand alone in our opinion upon this subject. Puzos (*Mem. de l'Acad. de Chirur.* Vol. I. p. 203,) declares that neither pain nor hæmorrhage necessarily produces abortion. La Motte (*obs.* 305) gives an instance where the woman went her full time after the orifice of the uterus was considerably dilated. And above all we may cite Mr. Burns himself for an example most strictly in point. (*Princip. of Mid.* ed. 2d. p. 195, in a note.) He relates with seeming belief that cases have occurred of twins, one of which has been expelled, while the other remained, and the “action of gestation,” as he happily terms it, was still maintained to the proper period. Now this is demonstration that after muscular action has been universally established, it can be suspended for a considerable time: if this be so under the circumstance of one fœtus being expelled, and the uterus by a cessation of action shall permit a second to remain until the

proper time, we *a fortiori* should expect it when the uterus is not so extensively or powerfully excited. Besides, we might urge cases related by both Mauriceau and La Motte, where the uterus was emptied of its waters, and yet the women went their full time, though they were not within six or seven weeks of it.—In these instances the uterus could not fail to have contracted. We however must fully agree with Mr. Burns, that where the “action of gestation” has ceased, it would be unavailing if not injurious to attempt the preservation of the ovum—for it *must* sooner or later be cast off. Denman is also of opinion that uterine contraction can be subdued. He says, “that experience has fully shown, that women who have had not one, but repeated discharges, with *considerable and regular pains*, have gone to their full time.” Introd. to Mid. p. 472. Francis’ Edit.

The remote causes which we have hitherto been tracing, may with much propriety be considered as contingent or accidental in their application and influence. But there is one still remains to be noticed, which must be regarded as absolute in its effects, whenever it may chance to exist—we allude to the implantation of the placenta over the mouth of the uterus.

The knowledge of this particular location of the placenta is of modern discovery—and perhaps Levret is the first, who decidedly taught this doctrine. Mauriceau, La Motte, and others before his time, met with the placenta in this situation, but they all believed it was a mere precipitation of this mass, after an entire separation from the fundus of the uterus. The whole process of generation is involved in such complete obscurity, that conjecture is constantly made to supply the place of facts, or of well ascertained processes. It would seem that the daring, or hardihood of the theorist was augmented in proportion to the obscurity of the subject, or the difficulty of ascertaining truth—hence we have nothing to rely upon but conjecture, on the manner in which the placenta becomes situated over the os uteri—nor shall we perhaps ever be more enlighten-

ed than at present upon this subject. Generation with all its attendants most probably will ever remain among the inscrutable arcana of nature.

Lereux,\* says “Lorsque l’œuf humain fécondé a parcouru le trajet de la trompe, et est tombé dans la matrice, il se trouve dans une cavité que est beaucoup plus ample que le canal d’où il sort. Son pedicule, qui doit former le placenta, *et qui est sorti le dernier de la trompe*, reste le plus ordinairement supérieur; cependant, comme l’œuf est encore flottant, le pedicule peut se tourner par quelque accident plus ou moins inférieurement.”

Mr. John Burns† follows Lereux very closely in his conjectures, or rather his assumption of facts upon this subject. He says, “as that part of the membranes of the ovum to which the embryo is attached, generally enters last, it follows, that the placenta will be formed originally over that part of the uterus where the tube enters the decidua, at that spot joining with the chorion to form it. But in some instances the case is reversed, and the embryo enters foremost, the rest of the membranes following it. When this happens, then the inner layer of the decidua which was stretched across the orifice of the tube, and which is afterwards to become the decidua reflexa, will contribute to the formation of the placenta. In this case, by the distention of the ovum, and the yielding of the decidua reflexa, the placenta will come at last to be inserted over the mouth or over some inferior part of the uterus.”

In this manner do these writers account for this unnatural situation of the after birth—The only difference in their views is, that Lereux, not understanding the nature of the decidua, or perhaps ignorant of its existence,‡ sup-

\* Obs. sur les pertes de sang, p. 13.

† Gravid uterus, p. 153.

‡ It is remarkable (so far as we at present recollect) that none of the French physiologists have faith in Hunter’s description of the decidua. Baudelocque, Meygrier, and Gardien declare, if it exist at all, it is only in the early months of gestation, and then perhaps only observable towards the lower part of the uterus. From our own observations, we have no hesitation to declare its existence, but not precisely as laid down by either Hunter or Burns,



posed that the ovum, after it was deposited in the uterus, was unconfined, or rather floating in its cavity, and might in consequence of this, by some accident turn its "pedicle" which was to become placenta, downwards; though it generally remained upwards, and thus become situated over the os uteri, while Mr. Burns supposes the portion which is to constitute this organ, enters the uterus by some chance first, and thus will have or assume this inferior situation.

From this it will be seen, that much is taken for granted, which, as it can never be proved, one conjecture may be as good as another, provided it is not found at variance with any well established fact. In this instance perhaps hypothesis can do as little mischief, as in any case with which we are acquainted—and as all practical ends are answered by the knowledge that the placenta is sometimes thus engrafted, we shall not attempt a refutation of it, especially as we have none better to substitute.

The order of development of the uterus is so uniform, that a deviation from it can only result from accident, or such a combination of circumstances as can very rarely happen. We can then with absolute certainty declare, that when the placenta is unhappily situated over the mouth of the uterus, a flooding towards the latter periods of gestation must be inevitable—hence the propriety of the term "unavoidable," for this kind of hæmorrhage.

During the first six months of utero gestation, the body and fundus alone yield to the distending power of the ovum : after this time the neck is called upon (if we may so term it) for its proportion, as the other parts of this organ seem to refuse any further supply—in consequence of which, it, in its turn, becomes distended, and in this act, a portion of the placenta is necessarily removed—and a bleeding, according to the extent of injury, or the number of vessels exposed or ruptured, ensues. After discharging more or less blood the hæmorrhage may cease, or be so reduced in quantity, as to excite little apprehension. But this is a false security—it is sooner or later renewed, either by a farther stretching of the neck, by the augmentation of

the ovum, or by the removal of the coagulum which had until now stopped the bleeding.

In this manner may things proceed until near the last stage of pregnancy—or the extent of separation may be such, or the size of the vessels exposed be so large, that the woman's life is instantly jeopardised, and from which she can only be protected by the most prompt, and decided remedies.

#### IV. *The periods of pregnancy at which hæmorrhage may take place.*

There is no period at which this may not take place, after the first month of pregnancy, since it is presumable, that after the fourth or fifth week, a union more or less strict is formed betwixt the ovum and the uterus by means of the chorion and decidua: it must therefore necessarily follow, that a separation may be effected, and a bleeding ensue. Until about the fourth, or between it and the fifth month of gestation, this accident may happen to any portion of the ovum, since up to this period, the placenta or what is to become placenta completely surrounds the ovum.\*

After this time, there is a portion of its surface that becomes transparent, and which uniformly augments in extent so long as the uterus continues to increase in capacity. This transparent portion is what is technically called the membranes—and towards the full completion of pregnancy they occupy a larger surface than the placenta, from which they appear to emanate. In consequence of this, there is a portion of the uterus from which no hæmorrhage can

\* We believe that the whole of the vascular covering, until the time above indicated arrives, is destined for, and converted into, placenta.—We do not believe any of these vessels become “blighted,” (Burns' gravid uterus, p. 196) as it would seem to be a work of supererogation—and we believe further, that there is a point in every ovum from which the transparent portion of the membranes proceeds, and that that point is always opposite to the insertion of the cord into the placental mass—this however is not the place for us to give our reasons for this belief, or we think we could satisfy on this head.

proceed, so soon as this transparent portion shows itself—and this portion relatively increases as gestation progresses—and of course, the source of flooding is confined to that part which is covered by the placenta—for all the remaining surface is lined by these membranes, and is incapable of furnishing such a quantity of blood as shall be denominated a flooding. We are aware that some have supposed the contrary of this, and declare that a separation of the membrane will yield sufficient blood for this purpose. But this is decidedly an error, since the whole transparent portion of the ovum is connected with the internal face of the uterus, by a very fine cellular substance only, in which very few red vessels enter, and these of very small size. Were this not so, we should have with every labour, a discharge of blood when the membranes protrude beyond the opened circle of the mouth of the uterus—which it is well known does not happen.

As a general rule, then, we find the risk from floodings in proportion to the advancement in pregnancy—because the vessels are larger, and will in a given time yield a much greater quantity of blood—though the chance of occurrence is in the earliest months. Puzos says that abortions under the fourth month are rarely fatal—and this observation is perhaps confirmed by the experience of almost every practitioner—provided a sufficiently early attention has been paid to it. It must however be confessed, that it is very difficult to establish any certain rule upon this subject—since we have seen as alarming symptoms attend an abortion at six weeks, as we have witnessed from a premature labour of the seventh month, or indeed at any other period. It may however with confidence be advanced, that alarming symptoms do not show themselves as quickly in the early as in the latter months, and of course we have much more time for the employment of proper remedies. We may farther observe that it is frequently from neglect that any danger arises in the early stages of pregnancy—this inattention may proceed from the aversion that many women feel to let any thing be

known that has any reference to their situation—from an ignorance of consequences, and from a long established opinion that a moderate discharge is useful, especially in plethoric women, &c. Time, of great consequence, is lost by this improper procrastination, and many an ovum has been cast off, attended with threatening hæmorrhage, which by early attention and proper care might have been preserved. Besides, the period of gestation has sometimes been permitted to lull the practitioner into dangerous security. Many of considerable experience maintain, that they have never seen danger from floodings before or at the period of three months. This is decidedly an error, and the sooner it is corrected the better. Whenever there is pregnancy with flooding there is danger—nor will the period of advancement, however short, protect of itself, against hazard. Of this, Mauriceau, La Motte, Gifford, &c. give us examples—and we may add, our own experience furnishes the same results.

We are not to wait for extreme symptoms before we act: it is this delay, which creates in most instances the danger—and sometimes it has its victim. The authors just mentioned, and more could easily be cited, have furnished us with cases, not only of great danger, but of death, before and at the fifth month. It is wrong then to treat such cases with indifference,\*—for though death may not be the consequence—extreme weakness, or a state of subsequent ill health, or the calling into action of some latent disease, may result from it. In another point of

\* Rigby treats this subject with great indifference; and the weight of his authority no doubt has tended to perpetuate, if not to establish, a most erroneous practice in the early months of gestation—he says, “the treatment of floodings that come on before the uterus has acquired any considerable size, must be very obvious, and the consequences of them at that early period of pregnancy are seldom to be dreaded, as, if the patient lose blood from the arm, be kept cool, and in a horizontal posture, and such mild astringents and anodyne medicines be administered to her as have been found by experience, to restrain discharges of blood, they will very frequently stop entirely, and the woman go on to her full time; and if this should not be the case, but the hæmorrhage should still increase, it will seldom increase to a degree to endanger the life of the mother.” *Essay on Uterine Hæmorrhage*, 5th Ed. p. 2.



view, it is highly important that early attention be paid to such cases—namely, the prevention of its recurrence : for after a woman has once aborted, there is no security against the second, and presently a habit of it is established, which the best advised means within our knowledge is not always sufficient to destroy.

We now come to the more important part of our subject, namely, the mode of treatment. In pursuing our inquiry into this, we shall endeavour to be as explicit as the nature of the subject will admit ; for we can but establish general principles, and modes of management, as every individual case will present a shade of difference ; and the treatment of this shade of difference, whether important or otherwise, must be very much left to the good sense and judgment of the practitioner. We, however, trust at the same time, that little embarrassment will be experienced, as the indications, and their fulfilment will be so decidedly pointed out, as to render the one pretty certain, and the other without much ambiguity.

With a view to perspicuity, we shall divide the disease we are considering into periods, and the remedies into their nature or supposed mode of action. We shall also consider the peculiarities of each period, and by this means more clearly and certainly establish the mode of treatment ; and at the same time, we shall give a sufficiently full consideration of each particular remedy—the period at which it is more especially indicated ; its mode of action, and the degree of confidence to be placed in it. By this method we hope to avoid the confusion that must necessarily arise from a more general consideration of our subject, as well as ascertain the positive and relative efficacy of the whole class of remedial agents.

In the division of this part of our subject, we shall nearly follow Dr. Denman's arrangement, as it embraces every essential variety of period at which hæmorrhage may be the consequence of utero-gestation, and may be considered under four heads : and 1st. That period of its occurrence in which the ovum is entirely surrounded by

the decidua and decidua reflexa, which will comprehend the first four, or four and a half months of pregnancy. 2d. Into all the remaining period of utero-gestation. 3d. Into the period between the birth of the child and the expulsion of the placenta. 4th. Into that which may follow the expulsion of the placenta.

This division is by no means an arbitrary one, it is founded upon principles and circumstances, that must not carelessly be lost sight of, if we wish either to understand the nature of the disease in question, or become acquainted with its most successful mode of treatment. For instance, until after the time pointed out in our first division, it would be highly improper under almost any circumstance to pierce the ovum with a view to the discharge of the liquor amnii, yet at the second period it may become an essential remedy. In the third, the woman's safety may depend upon the immediate delivery of the placenta, and the subsequent contraction of the uterus; while in the fourth, her life may be hazarded by having hastily withdrawn this important mass, when contraction was not present.

### *First Period.*

Until the period of four and a half months or even to the fifth, the ovum, when separated entire from the uterus, appears to be an ovular, spongy, fleshy mass; it bears evidence of attachment to the parietes of the uterus, in every point of its surface—and it would seem to show, that at any one part of this, it may be subject to separation, and the effect necessarily be, a solution of continuity of more or less vessels, and a consequent hæmorrhage. We have just intimated that this separation may be at any point of the ovum, but the effects will be in some measure different, as it may happen near the neck, or at the body, or fundus of the uterus. When the separation happens at the body or fundus of the uterus, before the blood can issue from the os tincæ, it must necessarily loosen the attachment between the spot of commencement, to the point at which the blood

issues ; it will therefore follow, that when this takes place, the chance of arresting a flooding, and preserving the ovum, must be diminished in proportion to the destruction of the connecting medium. But when the disunion takes place near the neck, the mischief will be less serious, though the discharge may be very abundant—that these two different conditions happen, there can be no doubt ; but the dignoses are far from being established ; indeed, we do not know that this distinction has ever been adverted to, or the marks which may distinguish each, ever been attempted ; nor is it perhaps of much practical importance that they should, since they do not require a difference of treatment.

We can, however, discover most probably the cause, why an hæmorrhage of great pertinacity and extent shall not be followed by abortion, while one of much less threatening aspect shall eventuate in it. It is a fact, notorious to every practitioner of experience, that, when hæmorrhage is accompanied by pain, the chance of preserving the ovum is diminished almost in proportion to its intensity ; while a flooding, which is not attended with it, constantly presents a hope that it may escape destruction, however profuse almost the discharge may be. Now, when a considerable separation takes place, as must be the case when it commences, at the upper parts of the uterus, pain will more likely occur, than when it happens near the neck—hence, we sometimes have pain before the blood issues externally ;\* the uterus in this instance suffers irritation from partial distention by the blood insinuating itself behind the ovum ; contraction ensues, and the blood is forced downward, and is thus made to separate the attachment between the ovum and uterus in its course, until it finally gains an outlet at the os tincæ.

In consequence of the uterus being excited to contrac-

\* Does this not seem to prove the separation to be remote from the os uteri, and serve to distinguish these two cases, especially in the commencement of hæmorrhage ? Dr. Bard says that “ when labour pains precede the discharge, miscarriage can seldom be prevented ; when they follow, they sometimes may.” Midwifery, p. 138.

tion, the friendly coagula which may form from time to time are driven away, and the bleeding each time is renewed, and accompanied, most probably, with an increased separation of the ovum, until, at last, from its extent, the ovum becomes almost an extraneous body, and is finally cast off. Now, the contrary of all this obtains, (at least for some time) where the point of separation is near the neck of the uterus, since much less destruction can happen, even though attended by pain, owing to the proximity of the denuded surface to the place of escape. The blood, instead of forming coagula above the point of separation, so as to irritate the uterus by distention, and increase the lesion, will find immediate issue through the neck of the uterus; and thus is removed a powerful and mischievous agent.

It must, then, clearly follow, that there are many cases of severe flooding, in which the ovum may be preserved, owing, in some instances, perhaps, to the part first separated by the action of the remote causes, and in others to the extent of lesion not being considerable. Now, as we have no unequivocal mark by which the one case shall be distinguished from the other, it becomes a duty in the management of all such cases, so to act, as if the ovum could be preserved\*—we have, ourselves, constantly acted upon this principle; and we have sufficient reason to believe we have been rewarded in a number of instances by success, when the hope was truly a forlorn one.

It might sometimes lead to happy results, could we certainly determine, *a priori*, where our endeavours would be

\* Mauriceau gives an instance, where the ovum was preserved, though there were frequent returns of hæmorrhage, until the third month. Obs. 60. Another, where there was almost a continual discharge for five weeks, and that, at times, very abundant. Obs. 678. To these we might add several instances of like kind in our own practice. Puzos declares that pain and flooding do not always produce abortion. *Mem. de l'Academ. tom. i. p. 211.* Kok declares women have gone their full time after severe hæmorrhages. See Pasta, p. 215. Dr. Bard says, "a discharge of blood from the womb, though a very frequent, and generally the most important symptom, is not necessarily followed by miscarriage." *Midwifery, p. 138.*



followed by success, and where they would not—much time might sometimes be saved, and much anxiety be avoided; but as this, with our present knowledge, can be ascertained but in few instances, it will be constantly erring on the right side, to suppose that the ovum may be preserved. As far as our experience goes, we must say, we have never been able to determine, with certainty, in the commencement, the cases that would eventuate in safety to the ovum, from those where it would be expelled. This has not been owing, we believe, to inattention to the subject; for we can truly say, much pains has been bestowed upon it; but is dependent upon the influence of a variety of causes, some of which are so occult as to elude our keenest search, and to others so little under control, as to render opposition entirely unavailing. We are told by some, that if the orifice of the uterus be open and clots freely pass, we may be sure the woman will miscarry.\* †

It has been supposed by some, that the *os tinæ* was always soon affected in cases of hæmorrhage threatening abortion.—Spigelius‡ declares he always found it open; but this neither accords with our own experience nor that of many others. Mauriceau declares he could not discover it open upon the most careful examination, in many instances of flooding.§ || We are rather of opinion, that the uterus has been supposed to be open, because of the expulsion of clots—but this is by no means true; for the coagula are always perhaps, but certainly much the most frequently, formed in the vagina, when an ovum occupies the cavity of the uterus. Of this, the most decisive

\* Mauriceau, &c.

† Pasta declares this not to be strictly true; he says there are instances of women going their full time, after severe flooding in the early months, where the uterus was sufficiently open to allow the finger to pass, and others, where abortion has ensued, though the *os tinæ* was for a long time closed. Pasta, *Traité des Pertes de Sang*, p. 28, vol. i.

‡ Pasta, p. 38, vol. i.

§ *Maladies des Femmes grosses.*

|| Dr. Rigby declares that women have died of hæmorrhage, without the uterus being much opened: but supposes in such cases it may be in a dilated state, p. 42.

proof can often be given, in the very early months of pregnancy, by a mere survey of the size of an expelled coagulum; many times it is five or six times the size of the uterine cavity, were this even not filled by the ovum. The conclusion then, that the uterus must be open to give passage to coagula, is not a correct one: nor is the supposition that after having dilated to give passage to a clot, it will immediately close again, more consistent with fact.\*

From this it would appear that little information can be derived from an examination of the state of the uterus in the commencement of a flooding; for the os tincæ may be completely closed for a long time, in some instances, and the ovum be eventually cast off; while in others, it may be naturally a little open, without offering additional risk to the embryo. But we may safely declare, where the neck of the uterus is distended, so as to resemble in feel the extremity of an egg, and however small the opening of the os tincæ may be, that there abortion will sooner or later take place. In this case the uterus is thrown into complete action, and the extension of the neck of the uterus just spoken of, is the effect of these contractions. There is another mark equally unequivocal, and to which we have already adverted, namely the cessation of morning sickness, a diminution of the abdominal tumour, and above all, the secretion of milk, followed by flaccid breasts. In both these cases, all attempts to save the ovum, by the administration of opium, bleeding, or other remedies, would be unavailing; the whole care should be directed to the state of the flooding.

Nor is the quantity of blood expended any positive evidence that abortion will take place, especially when unaccompanied by pain—for we have repeatedly seen a very large waste of it without any other evil attending; while on the contrary, we have witnessed the expulsion of the ovum with the loss of a very few ounces, when attended by pain.†

\* Pasta, p. 34, vol. i.

† Pain accompanying flooding should not make us abate our endeavours to save the ovum, but under the circumstances just stated above.

As a general rule, perhaps, it may be said, that those cases of flooding following any violence, more certainly end in abortion, than those which come on silently and slowly, without any apparent cause.

We should place no reliance upon the opinion that a moderate discharge of blood from the vagina during pregnancy is useful by removing topical plethora.\* On the contrary we should look upon every appearance of this kind with great suspicion, and treat it as if it were to become decidedly mischievous. Even the legitimate returns of catamenia, when there is reason to believe that the uterus is impregnated, should be treated with caution, since we cannot satisfy ourselves at first that it is merely a monthly purgation.† In all such cases where we have been consulted, we have directed as if it might be a discharge of an injurious character. In this we believe ourselves to be in the right, since no evil can result from the adoption of the advice, but from a neglect of it much mischief may ensue.

In all cases then, where there is a sanguineous discharge from the vagina of a pregnant woman, we should immediately treat it with the utmost care—all the essential indications should be instantly complied with, and no time should be lost by temporising.

The essential indications are, 1st. to arrest the bleeding; 2d. subdue pain if present; and 3d. prevent a recurrence of the hæmorrhage.

These three points are constantly to be kept in view, as the preservation of the ovum, or even of the woman, is dependent upon them. Therefore whenever a woman is seized with an hæmorrhage from the uterus, the sooner we can arrest it the better; every known remedy of efficacy is to be employed in succession, should the antecedent ones fail of success; and every advantage must be given to the means

\* Kok says that local plethora is a cause of hæmorrhage. See Pasta, p. 275.

† This is not the place to defend the opinion of a woman menstruating after impregnation; we shall only say at present, we are abundantly convinced of the fact.

by the patient and her attendants, by a strict adherence to the directions enjoined. It would be in vain for the physician to prescribe, if either the patient or attendants run counter to his instructions ; and in no case perhaps is this observance of more decided consequence than in the complaint we are now considering.

One of the first steps to be taken is to command the most perfect rest of body, and of mind as far as may be practicable. The patient should be placed upon a matrass, sack-bottom, or even floor in preference to a feather bed. The room should be well ventilated ; the patient very thinly covered ; her drinks of the mildest kind, such as toast water, cold baum tea, lemonade, ice water, &c.—no stimulating substance of any kind should be permitted. Care should be taken, even in the administration of food and of drinks, that the patient be not subjected to exertion to receive them ; they should be given to her while in an horizontal position. Her food should also be of the same character with her drinks—thin sago, tapioca, gruel or panado—in neither of these should wine or any other liquor find admission ; they can be rendered agreeable by lemon juice, sugar or nutmeg. All animal food, or the juices of them in the commencement of flooding, should be forbidden. Let whatever is given, be given cool. Absolute rest of every member of the body should be enjoined.

The officiousness of nurses and of friends very frequently thwart the best directed measures of the physician, by an overweening desire to make the patient “comfortable.” This consists in changing of clothes, “putting the bed to rights,” or altering her position ; all this should be strictly forbidden. Conversation should be prohibited the patient, and all unnecessary company excluded. Much mischief is frequently done by the injudicious talk of bystanders, who delight for the most part in the marvellous, and but too often relate the histories of cases which are every way calculated to appal the already but too much alarmed patient ; this kind of gossiping should be peremptorily forbidden,



even at the risk of giving offence, rather than permit it to the certain injury of the sick.

Having established a proper system for the repose of the patient and the government of the attendants, we should next determine the propriety of blood letting—this becomes very often of high importance, especially at this division of our subject; plethora is an usual attendant at this time, nay, may be, as we have hinted, the very cause of the alarm. Blood should be taken from the arm in a quantity proportionate to the exigency, remembering we do little or no good by the operation if we do not decidedly diminish the force of arterial action; let the pulse rather sink under the finger than otherwise; its repetition must be regulated by circumstances, recollecting, however, that hæmorrhage is sometimes maintained solely by exalted arterial action; as the following case will very clearly show:

We were called to Mrs. B. in January, 1796, who we found much exhausted by uterine hæmorrhage, in the fourth month of gestation. She had several days previous to our visit, returns of flooding, but were but little attended to. The usual means were now employed, and for the time being, the discharge was arrested—this was early in the morning of the 16th. She remained very well until 5 o'clock, P. M. at which time she had another return of flooding; we were instantly sent for, and living but a few steps from the patient, were very quickly at her bed side. She was found to be flooding very rapidly; the pulse was very active; and the eruption of blood appeared to be preceded by a slight rigor, followed by high arterial action; she was instantly bled from the arm, and the abdomen covered with ice and snow until there was a reduction in the force and frequency of the pulse; so soon as this took place, there was an abatement of the discharge; this condition was followed by slight alternate pains in the back, shooting towards the pubes. Forty-five drops of laudanum were now given, and strict injunctions were left that the patient should be kept as quiet as possible, and in case of return of the flooding, that we should be instantly

apprised of it. 17th, A. M. The patient was found free from fever and almost free from discharge; in this way she continued until about 5 o'clock, P. M. when the whole scene was renewed, as mentioned before; she was again bled; subjected to the application of the ice; and the laudanum was repeated for the same reasons as yesterday. 18th, A. M. 8 o'clock, was called suddenly to my patient, as she again had a return of fever, with hæmorrhage; she was again bled, &c.

In this manner did matters proceed for several days; it was found now, that the arterial exacerbations observed no regular period; but whenever they occurred there was uniformly a return of the flooding, and none but during this state of excitement; with a view to interrupt this condition, or to abridge it as much as possible, we placed a young gentleman at the patient's bed side with orders to bleed the moment he perceived an increase of pulse; this was accordingly done, and from each bleeding there was a decided advantage. The loss of five or six ounces of blood was sure to put a stop to the uterine discharge in the course of a few minutes, and sometimes would prevent its appearance when very promptly used. By proceeding in this manner until the 23d, the patient was entirely freed from this distressing complaint. She was bled seventeen times, and lost, by computation at the time, 110 ounces of blood in the course of seven days. She gradually gathered strength, and was safely delivered at the proper time.

The acetate of lead should now be given in doses, and in frequency, proportionate to the violence of the discharge. From two to three grains guarded with opium, may be given every half hour, hour, or less frequently, as circumstances may direct: or in case the stomach be irritable, a very efficient mode of exhibiting it is per anum—twenty or thirty grains may be dissolved in a gill of water, to which will be added a drachm of laudanum: this must be repeated pro re nata. If pain attend, more opium should be given than if there be none; and this must be repeated until a decided impression be made upon the uterine con-

tractions, or until its exhibition appears totally unavailing. Should the discharge be profuse, the application of equal parts of cold vinegar and spirit of any kind, may be applied to the region of the pubes ; or, what is still better, a large bladder two-thirds filled with ice and water.

The discharge from the vagina, when very profuse, will not always yield however to these remedies ; and if it does not, it will very soon become highly alarming. To save even a few ounces of blood is a duty, and sometimes is highly important : should then the means just recommended fail in moderating or stopping the threatening symptoms, no time should be lost in employing the tampon. The best we have ever used is a piece of fine sponge of sufficient size to fill the vagina. It should have pretty sharp vinegar squeezed from it several times with a view to cleansing of it, as also that it may be imbued with this acid ; it should then be introduced into the vagina, and suffered to remain until its object is answered.

Previously, however, to the introduction of the sponge, it will be well to examine the state of the os tinæ ; the condition we may find this in, will very much govern our decision and prognostics. Should it be found entirely closed and of its original shape, we may, notwithstanding the profuseness of the discharge, and even the presence of pain, still entertain a rational hope of preserving the ovum ; but if on the contrary its form be altered and the mouth opened, we are pretty certain it will be sooner or later cast off. But neither of these conditions are to affect our conduct as regards the bleeding ; for this is to be staunched though we are certain the embryo will be lost. Much error is committed sometimes under the impression that the ovum must be expelled, and that nothing can be done advantageously for the ovum until this is effected. We have known a hæmorrhage suffered to continue almost to the complete exhaustion of the patient, because pain was considered essential to this end ; though with each return of which, a large coagulum would be expelled ; or the discharge has been augmented by improper attempts to aid its expulsion. Both

of these mistaken methods cannot be too severely reprehended—one for blameable supineness, and the other for rash interference. Whatever may be the rapidity of discharge in such cases, it is ever under command, so far as our experience will warrant the assertion, by the use of the tampon. It should be instantly resorted to, and its effects will be as quickly perceived. If the ovum can be preserved, we save a prodigious expenditure of blood : if it cannot, we not only do this, but obtain a most important truce, during which time nature achieves the separation and the final expulsion of the ovum, without the farther exhaustion of the patient. For Leroux tells us that when the uterus is opened the tampon is not only useful in stopping the discharge, but in stimulating the uterus to successful contraction.\*

We deprecate with much earnestness, frequent and unnecessary touching. This is not only injurious by fatiguing the patient, but by removing coagula that may be important to the stopping of the hæmorrhage. This should therefore always be avoided, but at such times as it may become necessary to ascertain whether the mouth of the uterus be yielding to the influence of pain. It therefore can only be necessary in such cases as are or have been accompanied by uterine contractions. We also must seriously forbid all attempts to remove the ovum, so long as its greater bulk is within the cavity of the womb, lest we break through its covering and evacuate the liquor amnii. We must let no false theory get the better of multiplied experience ; all of which goes to prove the impropriety of such a procedure ; for it is agreed by the most enlightened men upon this subject, that it is mischievous to effect it, and unfortunate when it happens spontaneously. The reason is obvious. The embryo is expelled, and its involucrum is retained ; the consequence is, that the flooding is by this means perpetuated, and much pain and inconvenience, if not danger, is experienced before it is thrown from the uterus. We must therefore repeat it as a rule, that the ovum is never to be



pierced before the commencement of the fifth month,\* unless the flooding is very profuse, the pains very urgent, and the os uteri pretty well opened.

We are aware in this advice we depart from the very high authority of Baudelocque, (and with whom it is not very safe to differ) as he recommends this should be done always after the third month, provided the membranes do not tear of themselves. But multiplied experience has convinced us, that it is safer to preserve them, so long as the os uteri remains closed, be the pains ever so frequent or powerful, or the flooding ever so profuse, for the one may be diminished by opium, and the other arrested by the tampon. And if no pain attend, it almost becomes criminal to do so, since the ovum may, by the use of the tampon and the other remedies above suggested, be preserved. We have ever found, that, in such cases, much effort was required to expel the secundines; nor need we be much surprised at this, when we recollect the strong disposition the uterus has to close at this period of utero-gestation. Indeed, we have repeatedly witnessed the most alarming floodings from this cause; and we are certain that it was owing to the presence of the placenta, as the discharge always ceased so soon as this mass was removed. When the hæmorrhage is thus maintained, we should remove the placenta as quickly as possible; but here is the difficulty. At the early periods of pregnancy, which are comprehended within the first five months, the uterine cavity is too small to admit the hand, or a couple of fingers, or even one; therefore any attempt to deliver it by the hand alone will almost always fail. If this mass is entirely within the uterus, or even nearly so, the os uteri will be found most generally so much closed, even at the fifth month, as to prevent the introduction of the fingers so as to hook down the placenta; and as we descend from this to the second month or lower, it will be naturally so small as to prevent the intro-

\* Burton, and some others, advise the rupturing of the ovum even at the second month: than this, nothing can be less conformable to either sound reasoning or good practice.

mission of even one. When this is attempted (by the inexperienced especially) it is sure to eventuate in disappointment. Sometimes a portion of the placenta is felt without the os tincæ. If its greater bulk be so situated, we can sometimes remove the whole of it by pressing it between two fingers and withdrawing it, and thus put a stop to the discharge; but we are rarely so fortunate. In such cases we have employed, with the most entire success, a small wire crotchet to bring it away. This instrument is very simple in its construction as well as in its mode of action.\* The manner of using it is as follows: The fore finger of the left hand is placed within or at the edge of the os tincæ; with the right we conduct the hooked extremity along this finger until it is within the uterus; it is gently carried up to the fundus, and then slowly drawn downwards, which makes its curved point fix in the placenta; when thus engaged, it is gradually withdrawn, and the placenta with it. The discharge instantly ceases, in every case we have had occasion to use it. In every instance to which we here refer, we are persuaded that it preserved the woman. In illustration of what has just been urged, we will relate one case of several that has fallen immediately within our notice.

We were called to Mrs. H—— on the 3d August, 1807, who was flooding at the third month of pregnancy; pains were frequent and violent; large doses of the acetate of lead and opium were ordered, together with cold applications externally—the mouth of the uterus was a little open, and the ovum protruding; quiet cold drinks, &c. were ordered, and we took our leave. Returned at twelve o'clock, three hours after our first visit—the hæmorrhage not abated; the pains increased; the os tincæ more dilated, and the ovum more tangible. At three o'clock, P. M. the ovum opened spontaneously, and the embryo escaped—flooding

\* We have given a drawing of this instrument upon a reduced scale; the reduction is one third. We consider this much more simple than the pince à faux germe of Levret, recommended by Leroux and Baudeloeque, or that of Burton, commended for the same purpose.

violent; pains trifling; syncope frequent; pulse very small and quick; the placenta in part engaged in the os uteri—a stimulating injection was ordered, with the hope it would bring away the placenta. Four o'clock, P. M. the injection failed in the object for which it was given; hæmorrhage continues; syncope frequent; pulse scarcely perceptible. The placenta was now removed by the wire crotchet; the flooding ceased instantly; the subsequent symptoms were very mild.

Sometimes, when the ovum is ruptured, and the embryo has escaped, and left its involucrum behind, the hæmorrhage may not be violent, but may be of long continuance, at least as long as this mass may remain. In such cases, where time is not so precious to the safety of the woman, we have, in several instances, administered the ergot in twenty grain doses, with very decided and prompt advantage.

The peculiarity of this period consists in the ovum not having the transparent membranes formed; and the practice founded on this, as a general rule, is never to break the walls of it.

#### *Second Period.*

This comprises all the time from the fourth and half, or the fifth month, to the entire completion of utero-gestation. The woman is liable to hæmorrhage during all this period, by the action of any of the remote causes already enumerated; and in proportion to the advancement of pregnancy, is the risk from flooding, as the quantity of blood thrown out in a given time is, *cæteris paribus*, greater and more difficult to arrest. When a woman is, therefore, attacked with a discharge of this kind, however moderate it may be in its commencement, we have no kind of security against its increase at any after moment—she is to be carefully watched and most fully advised. We should insist upon her compliance with the rules we have just directed for the first period; and employ the remedies there proposed as early as the nature of the case may require.

We have already intimated, that an hæmorrhage from the uterus during pregnancy can only happen from a portion of the placenta being detached; it will follow, that the issue of blood will be in proportion to the extent of surface so exposed; to the advancement of pregnancy, and the force of the circulation. Now, as the advancement of pregnancy is greater in this, our second division, than in the first, the chances for a more profuse discharge of blood are increased in an equal proportion; hence it is agreed upon all hands, that the risk the woman runs is very great; so great, indeed, sometimes, as to be very speedily fatal, since we can have no influence over the extent of separation of the placenta, nor always have control over the force of arterial action.

The indications, however, are precisely the same as in the "first period;" but their fulfilment is not always effected after the same manner. Practitioners are by no means agreed as to the precise mode of arresting the hæmorrhage, though they all agree as to the necessity of the most decided and prompt application of remedies for this purpose; they may, therefore, be divided into three classes. The first of which, relies upon the exhibition of internal remedies and external applications. The second, depends upon the administration of medicine, and the use of the tampon; while the third insists there is no safety but in immediate delivery.

For each of these modes, high authority can be cited; and the young practitioner, pursuing his inquiries into this subject, becomes perplexed by their discrepancy—he hesitates between the different plans, and the period of his uncertainty is a loss of valuable time, or he adopts one the least suited to the case. With a hope, therefore, to lessen this embarrassment as much as our experience will warrant, we shall as briefly as possible weigh the merits of each of these plans.

Those who recommend the first plan, seem to do it upon the principle, that as great and as certain a mischief will arise from the employment of either of the two other me-



thods, as can result from the hæmorrhage ; hence they condemn the tampon, and deprecate delivery. But in doing this it is evident they have taken but a very superficial view of the subject, or been too much governed by prejudice. That both these methods can and have been abused, we readily admit ; but that they are as mischievous, when properly and timeously employed, as an unrestrained flooding, we cannot by any means agree to—but more of this presently. The practice of the first class must, by every man of experience, be admitted to be both feeble and uncertain ; nor can we ever recommend it to be exclusively relied upon in any threatening case. In moderate uterine discharges, alum, the preparations of lead, digitalis, and the external application of cold, together with astringent injections per vaginam, &c. may very often succeed ; and hence it is our uniform practice, to exhibit the acetate of lead, either by the mouth, or per anum (when the stomach is disturbed,) in cases of this description ; in a word, treating them in every respect as we would the mild ones in our “ first period.”

But what reliance can be placed upon these comparatively feeble remedies in those cases of hæmorrhage which threaten the life of the patient in a very short period of time—cases where the woman has drained off by far the larger portion of her blood ; where there is syncope, convulsions, and an extinguished pulse ? Can any man reconcile it to his conscience to stand by, waiting the success of a few grains of alum, or of sugar of lead, or of a few drops of the tincture of foxglove, while the woman’s life is rapidly passing away with the escaping blood ? In such cases, success can only attend either of the two or both the other methods, and to these two we must direct the attention of the young practitioner in every case of menacing appearance. Yet we are told of success attending the other, in some desperate instances.

Of the effects of alum in severe cases, we can say nothing from our own experience ; but from what we have witnessed in those of a milder kind, we should not be

tempted to place upon it much reliance ;—if given in small doses, it is insufficient to the end ; and when given in larger quantities, it has ever, in our hands, deranged the stomach so much as to be rejected ; and of digitalis we can say nothing in any case. But as this remedy is recommended by Mr. Burns,\* for floodings accompanied with increased arterial action, it may deserve great confidence to be placed in it ; but for ourselves, we should not be much tempted to employ it ; not for its want of power over the circulating system, but from its general unmanageableness, and the permanency of depression it sometimes occasions. Of the sugar of lead we have a much higher opinion. This has been considered by some as a new remedy ; but we find it was long since recommended by Etmuller, Friend, Kok, &c.—the two former in the form tinct. antiphthisic. and the latter in injections, combined with vinegar, per vaginum. Its effects are for the most part prompt and useful ; and we constantly regard it as an important auxiliary. We have given it liberally, and often with the most decided advantage—and we very rarely fail to employ it in addition to our other means. It can be given by the mouth in the quantity already mentioned, or by injection, as before suggested. We have never, in cases of this kind, placed any reliance upon injections into the vagina, for several reasons. 1st. Because they are very inconvenient in their exhibition, and especially as they must necessarily be rejected very quickly, and thus add to the discomfort of the patient, by wetting or raising her. 2d. Because their effects are both uncertain and transient. 3d. Because they may prove injurious by disturbing the patient, or by the removal of a useful coagulum. After delivery we have sometimes thought them useful, but never to the extent we are led to suppose by some.

It is then our uniform practice in every case of flooding during pregnancy of threatening aspect, or where from the rapidity of the discharge the woman's strength would quickly

\* Principles of Midwifery, p. 289.

be exhausted, to use, in addition to the means just mentioned, the tampon. We have already said we have found fine sponge the best of any we have yet employed—but where this cannot be procured, fine flax or very well picked tow, or old linen, may be substituted. When the latter substances are chosen, they should be used in portions of moderate size, and well moistened with sweet oil or melted lard—they should be introduced one by one until the vagina is completely filled; the whole may be secured by a compress and T bandage. This latter precaution is not necessary when a sponge is used, if the piece be of proper size. It is introduced, from its compressibility, without the least inconvenience, wetted with vinegar; and we believe it promotes coagulation quicker than any other substance we have hitherto employed, from its numerous cells quickly giving passage to the finer parts of the blood. It almost instantly puts a stop to the hæmorrhage; and we are well persuaded in some instances we have been entirely indebted to it for the preservation of the woman's life.

As this remedy is so confidently recommended by us, it may be well, as it will appear a novel one to many, to say something more upon the subject, and endeavour to obviate the objections which have been urged against it by several respectable practitioners. The tampon is by no means a remedy of modern invention. It may be traced, as we are informed by Pasta, in several of the ancient authors;\* but Hoffman gave the first clear account of it, and it was used many years ago by Smellie. <sup>Lug</sup>Leroux, however, is its great defender; and coming from a man of his experience and candor, we felt at once a confidence in it, and first employed it upon the strength of his recommendation. He has given us many cases, where its effects were very decidedly useful, and where it would seem in all human probability, that death would have been the inevitable consequence had it been omitted.

\* Pasta says, it was employed by Hippocrates, Moschian, <sup>Lug</sup>Leroux, &c. Pasta, vol. i. p. 277.

It is truly a matter of surprise, nor are we able upon any conjecture to account for its not being considered by the British writers as a remedy in uterine hæmorrhage, from the time of Smellie to that of Burns. It is true, indeed, it is mentioned by Dr. Denman, but he evidently places no reliance upon it; nor does Rigby lay the smallest stress upon its efficacy; he merely says "that should a case occur in which the uterus is too small to admit the hand, and yet the discharge so considerable as to endanger the life of the patient, before nature, by her own efforts, seems likely to effect an abortion, the method recommended by Leroux might, *I think*, with propriety be adopted."\* Dr. Merriman merely mentions it *en passant*; and says he has had reason more than once to think it had been prejudicial†—but he mentions its employment only in hæmorrhages succeeding the expulsion of the placenta. But Mr. Burns makes honourable mention of its efficacy, and seems to place no inconsiderable dependence upon it. Since the publication of his work upon midwifery, others have regarded it as a valuable mean in arresting flooding; so that at this time it appears to have awakened more attention than it formerly did.

The objections which have been urged against the tampon are: 1st. The danger of local inflammation from the use of the vinegar.

To this it may be answered that were vinegar even attended with this effect, it would be no objection to the tampon, since it would be easy to omit its employment—but our own experience warrants us in saying, we have never in a single instance witnessed it: nor is there the smallest probability of such a consequence following its employment.

2dly. Making a dyke for the effluent blood, it may convert an open hæmorrhage into a concealed one.

To this we can with much confidence declare, this can never happen in the cases comprehended in the two first

\* Treatise, p. 62.

† Synopsis, James' ed. p. 178.



divisions of our subject, since the uterus at both these periods is occupied by the fœtus, and the vagina by the tampon—it is evident, therefore, that no more blood can accumulate than will fill the interstices in the vagina, or the room made by the blood raising the uterus higher in the pelvis: the first of these must be few, if the vagina be properly filled by the sponge or any other substance used as a tampon: and the second must be very limited, since we know this mechanical effect must quickly be at its maximum—for the uterus when thus loaded cannot be made to ascend very high into the abdomen; consequently much blood cannot be expended.

3dly. Coagula may become putrid, and thus do mischief by their decomposition.

This objection is of less weight than either of the two former, since it is by no means necessary to continue the tampon so long into the vagina as to run any risk from putrefaction, twelve or fourteen hours being the longest periods necessary for its presence; at the expiration of this time, Leroux and others advise its removal, and in this we concur with them.

4thly. It may occasion a rupture of vessels, agreeably to Van Swieten, by stretching the ovum from the sides of the uterus.

This cannot happen but in those cases where the os uteri is pretty well dilated; and even in these, such effect is very problematical—but in this latter case, were it even true, no evil could result, since the fœtus under such circumstances must sooner or later be thrown off, as the uterus would be certainly thrown into action—we shall presently say, we regard this remedy as of high utility in cases of this kind.

5thly. It will, according to Kok, always be followed by the expulsion of the fœtus, as it always provokes uterine contraction.

We need not be much surprised at this assertion of Kok, since he supposes the orifice of the uterus is also to be

plugged ; now as we never consider this necessary, we can not consider his objection of much force.\*

But although Kok thinks expulsion will follow the use of the tampon, he still bears honourable testimony to its efficacy in arresting hæmorrhage in threatening cases; he says, "Ce procédé (namely the tampon) est infiniment préférable à ceux de Mauriceau, et de Puzos ;" (that is, rupturing the membranes) "il n'augmente jamais l'hémorrhagie, il la ralentit et l'arrete souvent."†

The mode of action of the tampon in stopping hæmorrhage, is precisely that which nature employs when she alone effects the same end. A coagulum is formed from the tampon to the mouths of the bleeding vessels, and thus puts a stop or very much diminishes the farther issue of blood. It would seem from all we know upon this subject, that there is a strong disposition in the cut or divided extremity of a blood vessel when at rest, or nearly at rest, to form a coagulum within itself for the purpose of putting an end to the farther issue of this fluid—hence the importance of coagula at the mouths of the bleeding arteries, the formation of which is the first step towards spontaneous suppression. Puzos‡ many years since had pretty nearly the same notion upon this subject ; he said that the coagula acted as corks to the mouths of the bleeding vessels.

It has been supposed by several, that after the suppression of an uterine hæmorrhage arising from a separation of a portion of the placenta, a re-union takes place between the separated parts ; we do not believe this, as the connecting medium between the uterus and placenta must necessarily be destroyed, or so much injured as to render it improbable it can ever serve the same purpose again, after the same manner—the vessels are certainly ruptured by the se-

\* Pasta, p. 279.

† Pasta, p. 277.

‡ "Ces sages precautions ont suspendu souvent, et quelquesfois ont fait cesser des pertes de sang accompagnées de petits caillots ; non pas en soudant, pour ainsi dire, à l'interieur de la matrice les portions du placenta séparées, mais en donnant le temps au sang arrêté à l'embouchure des vaisseaux de s'y cailler, et d'y former de petits bouchons moulés sur leur diametre, capables d'arrêter le sang."—Mem. de l'Acad. Tom I. p. 211.

paration or their extremities so exposed, that there is scarcely a possibility they can be again united and serve as a bond of union between these two parts. Nor is it more probable that a union would be effected by the effusion of coagulum lymph after the manner it is performed in many other portions of the body, since this most likely would be mischievous by its firmness. Did a union take place in this way, every woman who was thus situated would run the risk of an adherent placenta—besides, we have repeatedly been able to detect after the expulsion of this body the separated portion, in cases which had been preceded some time before by flooding; this betrayed itself by being very much darker than the other portions, and being covered by a very fine layer of coagulated blood.

The internal remedies for the suppression of uterine hæmorrhage, when successfully employed, must act in such a manner as to dispose the blood to a more speedy coagulation, or immediately upon the opened extremities of the bleeding vessels, so as to induce a contraction of them. Hence, the almost universal employment of that class of medicines called astringents, with the expectation of all being more or less efficacious. Leroux\* forbids them in uterine hæmorrhage, after delivery; but he does this upon a wrong principle; he says, “dans l’hémorragie utérine violente qui succède à l’accouchement, ils, ne peuvent être d’aucune utilité. Pour s’en convaincre, il suffit de se représenter la route qu’ils sont obligés de suivre avant de parvenir au lieu où leur effet pourrait être utile, le temps qu’ils mettent à parcourir ce trajet, et les changemens qu’ils éprouvent avant d’y arriver.”

In like manner from their mode of action, Leake† objects to the use of astringents or styptics in this complaint, and as we conceive, upon no better ground than Leroux; for we know, that certain of them, as the sugar of lead especially, sometimes produces the most decided effects, let the mode of its doing so be what it may. In many instances

\* Observations, &c. p. 200.

† On child-bed fever, vol. II. p. 301.

it seems to exert a control over the bleeding vessels, as prompt as the ergot does upon the uterine fibre; and from the extent and certainty of this action, we might be tempted, without doing much violence to the delicacy of medical speculation, to call its action specific. In a word, we may justly question, whether any internal remedy can be successful in uterine hæmorrhage, which does not exert an action somewhat specific.

Since, then, experience and observation have frequently taught us the value of certain applications or remedies in uterine hæmorrhage, it is proper they should be employed while ever their chance of success is probable, or even when they can be made auxiliary; but they should never be depended upon beyond the time it is usual to witness their effects, in cases requiring farther management—so, neither internal remedies nor external applications should be exclusively relied upon longer than is decidedly consistent with the safety of the patient; for neither astringents of any kind, nor the tampon can be availing in all cases—and when they fail there is but one resource, namely, delivery; the consideration of which, brings us to the mode employed by the third class of practitioners for stopping uterine hæmorrhage.

From the time of Mauriceau and Dionis, to the present moment, the number belonging to this class is very considerable; and if numbers were merely considered, the weight of evidence would be in favour of this practice. The want of proper knowledge in treating uterine hæmorrhage by other means, the fatal rapidity sometimes of its termination where rupturing of the membranes, or delivery was not performed, or where a feeble plan had been pursued; the occasional success of these plans, together with almost the certainty of uterine contraction after this organ is emptied, and the influence of this contraction in arresting the bleeding, has but too easily and too generally found advocates for its almost exclusive employment. Thus La Motte\* thought it impossible to restrain hæmor-

\* Traite des accouchement. Obs. 216.



rhage when the placenta was detached in part or entire, but by the extraction of this mass ; Dionis declared we should not defer the delivery of the fœtus, if blood in great quantity, and without interruption escaped from the uterus.\* Mesnard advised delivery if there was a flooding sufficient to cause fainting ;† and Heister‡ and Puzos,§ were of the same opinion, &c. &c. for it would be easy to multiply authorities to considerable extent to the same end.

The advocates for delivery as the only means of arresting hæmorrhage, may be divided into two classes ; the first, into those who paid no regard to the condition of the uterus when the operation was undertaken, but proceeded immediately to the extraction of the child. The second, into those who evacuated the liquor amnii, with a view to promote the contraction of the uterus, and by this means put a stop to the flooding—these last may be subdivided into three—1st. Those who paid no regard to the situation of the os tincæ when they ruptured the membranes ; but when this did not immediately succeed, by forced means entered the uterus with the hand, and immediately effected the delivery. 2d. Those who having torn the membranes and gained the feet, were contented to bring them to the orifice of the uterus, and then trust to the natural efforts to perform the delivery. 3d. Those who never pierced the membranes, but when the mouth of the uterus was either dilated or dilatable, and who after rupturing them, permitted them to escape gradually, and finished the delivery very slowly or waited for the efforts of nature.

From the improvements which midwifery has received within the last fifty years, we should not have expected to have met with an advocate for indiscriminate delivery in a modern writer upon this subject ; yet in Meygrier we find that advocate. As he is a late writer, and one of reputation, it is to be supposed his directions for the treatment of floodings, would comprise the best opinions, founded upon re-

\* Des operations, p. 249.

† Pasta, p. 170.

‡ Surgery, part 2d, p. 957.

§ Mem. de l'Acad. vol. I. p. 224.

peated experience ; and that any directions he might give as an author and as a teacher, would be the result of mature reflection and observation. But, instead of this, we find him recommending immediate delivery, whether the uterus be dilated or not—as this is a point of high importance, we shall give his words and reasoning upon this subject.

“ Enfin, quelsque soient les moyens que l'on ait employés pour combattre la perte utérine, lorsqu'elle est assez violente pour mettre en danger les jours de la mère et ceux de l'enfant, il faut de suite avoir recours à l'accouchement forcé ou contre nature. Le col est-il dilaté, ou pénètre sans peine dans l'intérieur de la matrice, on perce les membranes si elles ne le sont pas déjà, et on termine l'accouchement par les pieds. Le col est-il dur et fermé, on se sert du moyen si bien décrit par Celse, c'est-à-dire que d'abord on introduit le doigt indicateur seul, puis le doigt du milieu, ensuite le doigt annulaire, et successivement toute la main ; et, après avoir ainsi vaincu sa résistance, on se comporte comme dans le cas précédent. Je suis loin cependant de partager de l'opinion de quelques auteurs qui prétendent que, dans un cas de perte, même des plus abondantes, il faudrait temporiser, en cherchant à pénétrer dans la matrice, on trouvait le col dur et fermé ; dans la crainte, disent ces auteurs, de le contondre et de causer de la douleur à la femme. Mais pourquoi attendre ? La femme est en travail ; et quel est l'espoir de l'homme de l'art ? Est-ce que la douleur momentanée causée par la dilatation du col, et même sa contusion, doivent être mises en parallèle avec le danger qui menace la vie de la femme et celle de l'enfant ? Non seulement on doit vaincre la résistance du col, lorsque celle-ci s'oppose à l'introduction prompte et facile de la main de l'accoucheur dans la matrice, mais les meilleurs praticiens n'ont pas craint de conseiller l'incision même de cette partie dans le cas d'une résistance invincible.”\*

\* Meygrier, Nouveaux Elémens, &c. p. 271.

This daring practice we confidently hope will never be pursued by any one, especially the cutting part: first, because it is contrary to all experience; and secondly, because it can never be necessary, but when the os tinæ is in a diseased state; a circumstance of such rare occurrence connected with the other, as scarcely to be called an exception.

That the most mischievous consequences have followed the practice of those who compose the first class\* just mentioned above, we have the authority of Pasta,† who deprecates the practice as both cruel and dangerous; of Kok,‡ who says he has seen it followed by inflammation of the womb; of Leroux,§ who declares it to be dangerous to both mother and child; of Baudelocque,|| who insists that nothing can justify the accoucheur who persists to deliver while the neck of the uterus retains its natural thickness and firmness. And we ourselves once witnessed death as the consequence.

The method pursued by the first division of the second class, is not free from serious inconveniences; and they are perhaps, scarcely inferior to the first, as the same violence almost is obliged to be committed. The plan of the second division of the second class, (which we shall in conformity with custom call Puzos' method) is far from being the one most conformable to the principles of the art, since in its performance *great* violence is frequently obliged to be resorted to. The objections to this scheme are, 1st. That every flooding during pregnancy is not necessarily followed by delivery; but if we adopt this method, it must sooner or later take place, to the perhaps certain destruction of the fœtus. 2dly. Because the mouth of the uterus may be so placed as to render this operation very difficult if not im-

\* Among the first class may be reckoned all the accoucheurs prior to the time of Mauriceau. To the second class, and the first division of that class, belong Mauriceau, Dionis, La Motte, Deventer, &c. &c. To the second division we may place Puzos, Smellie, Delourie, &c. &c. And to the third, we have Leroux, and most of the late writers upon midwifery.

† Vol. I. p. 132.

‡ Pasta, p. 276.

§ P. 241.

|| Vol. II. p. 90.

possible, especially when the uterine orifice is still very thick and rigid ; for Puzos\* himself confesses he was an hour or more before he could pierce the membranes ; and this was a loss of most precious time to the patient, as the flooding still went on, and he began to despair of the success of his method from the excessive loss of blood, and was fearful he should be obliged to have recourse to forced delivery. 3dly. That hæmorrhage does not always cease after the rupture of the membranes, but on the contrary sometimes only manifests itself at that time. 4thly. That the presentation of the child, and the presence of the placenta over the mouth of the uterus, will render this method ineligible. 5thly. It is sometimes impossible to make a forced delivery, especially from the fifth to the sixth and half month ; of this La Motte† gives an example, and Smellie‡ another—and we ourselves once saw a similar failure. And above all, they have not pointed out any alternative when their plan shall have failed.

It is then but upon the method of those comprising the third division of the second class, or those who never pierce the membranes, but when the os uteri is dilated or dilatable, that we can safely place reliance in cases of severe flooding.

It may be asked, what are we to do in cases of profuse hæmorrhage; at any period from the fifth month to full time when the discharge threatens the life of the patient, and when the os uteri is both closed and rigid ? Are we to silently witness her death, rather than employ some violence to relieve her ? Certainly not. If there really was no other remedy, forced delivery with all its disastrous consequences, might be justifiable : but as we have the power of plugging the vagina, and thus prevent the farther issue of blood, we should have immediate recourse to it : and this plan, so far as we have witnessed, has not yet failed ; and this experience is so supported by that of Leroux, as to entitle it to the utmost confidence. By this means time is permit-

\* Mem. Sur les Pertes, &c. p. 336.

† Obs. 452.

‡ Collect. 33. No. 2. Ob. 1.



ted to the natural agents of delivery for the performance of their duties, and this is done for the most part with both certainty and success. We could illustrate this by appropriate cases, but we have already exceeded the bounds we had prescribed to ourselves for the consideration of this subject. We shall refer to Leroux for farther confirmation.

The importance of the tampon is perhaps never so clearly demonstrated, as when it is employed in those cases where the flooding has proceeded to almost complete exhaustion—where every ounce of blood is of immense value. In such cases (before delivery) we have seen it arrest a profuse flow in almost a moment, and where the farther loss of a few ounces must have been followed by death. Syncope, and even convulsions, have ceased upon its application.

There is no greater error in obstetric practice, than the opinion that fainting is a desirable event. That it has been useful, *quo ad hoc*, must be confessed; but who in his senses would wait for this as a remedy in uterine hæmorrhage, since it can only occur from the extreme weakness of the patient? Who would wait for this forlorn effort of nature, when he could command a tampon? If the practitioner were absent during an exhausting profluvium, and learnt before he could exert his skill that the patient had fainted, he might suppose it to be useful *pro tempore*; but he should never look upon it but as a *dernier remède*.

Dr. Denman's opinion upon this subject is replete with mischief. It makes a young practitioner indifferent to the quantity of blood that is wasting, because a state of fainting has not yet come on—and when this condition does come on, he hails it as a most friendly visitation—forgetting that fainting is a decided proof of extreme exhaustion, and that his patient may never recover from it. Dr. D. emphatically calls it “a remedy provided by nature for averting the immediate danger of all hæmorrhages, and to prevent their return.” Who with this belief would not rather invite fainting than avoid it? But let us not be

deceived by terms. That a state of syncope favours or promotes coagulation, is agreeable to all observation ; but whether this arises from an inscrutable law of the animal economy, and for purposes entirely out of view, or instituted for the end assigned by Dr. D. may admit of much doubt. For, were it for this latter purpose, it would be much more advantageous to the individual to have it answered at a less expense, or at a period more suitable than the one at which it takes place—for, on such occasions, it would be much better to imitate nature in the end, than in the means ; and this is what is constantly aimed at, when we use lead, digitalis, the tampon, &c.

Again, we cannot agree with Dr. D. in his proscription of “cordials or stimulants,” in the state of extreme exhaustion to which women are sometimes reduced by floodings; we think we are as certain of the propriety of our practice in this instance, as we are of any other ; and we employ them, whenever the pulse is very much reduced, or extinct, the extremities cold, the breathing hurried and short, vision imperfect, and voice almost inaudible, with the most decided advantage. It is true, we administer them with caution, but with steadiness ; and in such quantities as shall neither offend the stomach, nor invite too much re-action. In this we persist, until there is evidence that the system will re-act—so soon as this appears, we desist from all stimuli, nor return to them, but upon a fresh necessity being created.

There is another position of Dr. D's arising from his particular views upon this subject, which, to say the least of it, wants confirmation—namely, that “during faintness, the advantage arising from the contraction of the uterus, is likewise obtained.” We have no hesitation in saying we have repeatedly seen precisely the contrary happen—we will illustrate this from one of several examples. Mrs. B. was delivered after rather a tedious, though pretty severe labour ; the placenta was in due time spontaneously expelled, and the uterus was well contracted. About half an hour after we had taken our leave, we were very sud-

denly summoned to Mrs. B's bed side, as she was extremely faint, and had lost considerable blood. We immediately commenced a brief friction with the hand upon the abdomen, and continued it until the uterus was felt firm under it—the discharge immediately ceased—in a few minutes after Mrs. B. told us again she felt very faint; at the same moment the uterus was found to become flaccid under the hand, and again there was a return of discharge—the friction was continued until the uterus was made to pucker itself up; the faintness went off, and every thing promised well, until another attack of syncope again relaxed the womb, and another gush of blood instantly followed; in this way did the faintness and relaxation of the womb follow each other for eight or ten times; but a perseverance in the friction and the exhibition of some wine and water, eventually overcame the disposition to faint and there was no farther return of the relaxation or of the flooding. Now, in this case, the state of faint was constantly followed by a relaxation of the uterus, so that when syncope arrests hæmorrhage, it must be more by the formation of coagula, than by effecting the contraction of the uterus.

And though it is strictly true, as Dr. D. asserts, that the uterus “acts or makes its efforts to act, in sleep,” and if we are to believe him and others,\* “sometimes even after death,” yet it does not prove that a state of faintness is favourable to this end. Indeed the uterus appears so independent, in many instances, of any condition of the other parts of the body, that it may be said, with the greatest propriety, to be governed by laws and conditions of its own, and over which other portions of the system do not seem to exert the smallest control. Who has not seen an alarming flooding from the inertia of this viscus, in a woman whose physical strength has been almost in excess? And on the contrary, witnessed its firm and secure contraction, where every other power almost has been exhausted by previous disease?

\* Baudelocque, Leroux, Kok, &c.

As it is confessed that after the failure of the remedies recommended for the suppression of hæmorrhage, the application of the tampon, &c. that there is but one means left in our possession by which the flooding can be arrested, and the life of the woman preserved—yet it may be asked, is there no condition of the patient in which it would be improper to attempt delivery, besides the rigidity of the os uteri. To this we answer yes—we would say, that a woman reduced to the last extremity of weakness, but with whom there should be a suspension of the discharge, should not be meddled with, so long as the hæmorrhage was kept in check. But suppose the same degree of weakness, with a continuance of the flooding, should we in such case attempt delivery? We have no hesitation in answering in the affirmative—but previously to the operation, the condition of the patient should be candidly stated to her friends; it should be undisguisedly declared, that no undue calculation should be made of the chance from delivery; but as it offers the only possible chance of relief, it should be adopted. We should be the farther encouraged to do this, as it now and then has happened that the woman has recovered, even contrary to all expectation.

Hitherto, we have said nothing of opium as a remedy in uterine hæmorrhage; the reason is simply this—it never in our hands has merited the smallest commendation, or met with the slightest success; of course we are not of opinion it deserves the encomiums which have been so lavishly bestowed upon it by Dr. Hamilton and others. We have read dispassionately, and with care, a review of Dr. Stewart's book upon this subject (the work itself we have never been able to procure) and have examined the cases detailed there; and we must for ourselves declare, we have not the slightest belief that the opium had the most remote agency in arresting the floodings, for which it was administered—the cessation uniformly appeared to be the result of the natural powers of the system in general, and of the uterus in particular. That it is frequently highly beneficial at any period previous to delivery in allaying



pain, and in this way putting a stop to further mischief, we most freely confess—but we can yield nothing more. We are not alone in this respect; Dr. Denman seemed to entertain a similar opinion; and Barlow has advanced the same sentiments.

It may be proper to say a few words upon the subject of cold applications; as no remedy has been more extensively employed, or more certainly abused. Cold as a mean to arrest flooding, is in almost universal employment; is usually one of the first resorted to, and the last that is abandoned—it has acquired so much popularity among the vulgar, as to render it unsafe to the reputation of a practitioner to omit it in his treatment of this complaint. But, though confessedly an agent of great power, it has nevertheless its limit of usefulness, and beyond which it should never be urged—its efficacy is entirely confined to its influence over the circulating system, by diminishing its vigour and abating its velocity. When these ends are answered, it is truly doubtful whether it should be further persevered in; at least its value is much diminished. It is our custom to employ it very liberally, and sometimes, if the case be urgent, at a very low temperature—in general the best mode of applying it is by a large bladder, as has already been directed—but in very sudden and alarming cases, we have found teeming it from an height upon the abdomen, to have a very decided preference, from the promptness and extent of its effects. But when the pulse flags, and the woman is much exhausted, we not only forbid it, but pursue the opposite plan, by having a warm blanket or other articles to supply its place. During the use of cold water, &c. to the abdomen, we are constantly in the habit of ordering warm applications to be made to the feet and legs; a bottle or jug of warm water well corked, is one of the best and the handiest—this last direction we are very particular never to omit, should the feet and legs be preternaturally cold. We also should be particularly careful not to wet the bed and clothes of the patient, if it can be possibly avoided, as it creates much inconve-

nience, without doing the least good—it will render the poor woman's situation extremely unpleasant, besides obliging her to be disturbed, that dry things may be substituted.

The injection of cold water, cold alum-water, the solution of the acetate of lead, the introduction of ice into the vagina and even into the uterus, &c. have all been practised, and it is said, with advantage. The merits of such applications must rest upon the authority of those who recommend them, for we are free to confess we have no experience in either of them, before delivery, nor should we be tempted to rely upon them in very pressing cases.

Leake\* is the most intrepid employer of cold we have met with ; he not only recommends its application in uterine hæmorrhage, but declares it as his opinion “ from repeated experience, that intense cold to the body is more to be depended upon, and will produce more salutary effects, than any thing else that can be devised.” Notwithstanding this high encomium and bold commendation from a man of great experience, we have our doubts, (and these doubts founded upon multiplied opportunities to ascertain the fact,) of the propriety of this practice, under any other circumstances than those just mentioned. He goes so far as to have the feet and legs plunged into cold water ; nor does he confine these applications to a particular state of the system ; his practice is predicated upon his theory of the action of cold upon the blood—he supposes the blood to be condensed by it, and is thus, by its greater thickness, prevented from escaping so readily from the vessels.

It would follow then, agreeably to this doctrine, that the greater the cold and the longer it be continued, the more decided would be its beneficial effects—but this, experience too often contradicts. It would seem to be a law of the animal economy, that almost in proportion to the reduction of the vital energy, is the disposition of the blood to coagulate—and it is but by effecting this reduction of vital power,

\* Leake on Woman, vol. ii.

that cold is useful in uterine hæmorrhage ; it will be then a necessary consequence, that cold can only be employed to advantage, or even with safety sometimes, where the vital energies require, or will bear diminution, and that this reduction should never be carried, as we have already intimated, to an extent that will prevent re-action, when this may become safe or necessary. Besides, we have very strong doubts of the power of cold, however long continued, to produce a "*condensation*" of the blood, (so long at least as life exists with any vigour,) in deep seated portions of the system, though entirely convinced of its influence over even inordinate excitement, by its action upon the skin and other portions of the body ; and also that when the reduction of temperature is carried to a certain extent, the blood will and does more readily coagulate ; but if it be persisted in beyond this, we may extinguish the already too much diminished excitability.

Again, after delivery, the safety of the woman is alone insured by the contraction of the uterus ; therefore whenever cold does not do this, its operation is totally unavailing, however much we may suppose the blood to be "*condensed*," or however disposed it may be to coagulate—indeed, in some instances we have witnessed, we have strong reasons for believing that contraction was prevented by the powers of life being too much depressed by the long continued, and persevering use of ice or other cold substances.

It may be proper to observe in addition to the remedies and modes of proceeding pointed out in this division of our subject, that in certain cases of uterine hæmorrhage, the forceps is the only means to be employed or relied upon. They are exclusively indicated—1st. Where the discharge is threatening and the labour is well advanced, but where the membranes have been long ruptured, and the uterus is firmly embracing the body of the child, or the head does not advance with sufficient rapidity to afford security. 2dly. Where the head is low in the pelvis, and has escaped from the orifice of the uterus—here turning must not be thought

of, however recent may have been the escape of the waters, or however moveable the head may be in the pelvis. 3dly. Where the uterine efforts are either feeble or suspended, and where the os uteri is sufficiently distended, but where the waters have been long discharged. 4thly. Where the head occupies the inferior strait, the orifice of the uterus sufficiently expanded, the waters recently expended, but where the natural agents of delivery would act too slowly for the safety of the patient. 5thly. Where the natural powers are incompetent to the sufficiently speedy delivery of the patient, owing either to the *mal* position of the head, or to such a disparity between it and the pelvis as shall prevent its timely expulsion.

*Hæmorrhage from the situation of the Placenta.*

We must now speak of that hæmorrhage which is so appropriately termed the “unavoidable,”\* and which, as we have already declared, arises from the peculiar location of the placenta. The first evidence of the placenta being over the mouth of the uterus, may declare itself so early as between the sixth and seventh months of utero-gestation; at this time the neck of the uterus begins to be stretched for the more complete accommodation of the fœtus—in consequence of this, a small portion of the placenta will be separated from the uterus, which will be followed by a discharge of blood, commensurate with the extent of the lesion, and the size of the vessels involved in this destruction. This discharge may, by proper management, be made to cease; nor will it return until the uterus and placenta are again forced to separation—then another perhaps

\* We are indebted to Dr. Rigby, for this term; he has written a valuable Treatise upon this subject, though anticipated by Levret, in the discovery that the placenta might be originally fixed upon the os uteri. But it would appear it was an original suggestion with him, as well as with Levret, for he assures us at the time he promulgated this doctrine, (and no one will doubt Dr. Rigby’s word,) he had never seen that author’s work, and that his “ideas upon this subject were derived from his own personal observations and experience.”—*Essay on Uterine Hæmorrhage*, p. 13.



slight hæmorrhage ensues, which may also cease, and not be renewed until the last period of pregnancy; or there may be, as happens sometimes, a constant stillicidium of a bloody sanies.

Dr. Rigby, who is considered the highest authority upon this subject, does not seem to have bestowed as much attention to the condition of the patient before the full period of utero-gestation, as he did to the consequences when that time arrived, or he would not have held the doubtful language he did when speaking of the "time and manner" in which the "accidental" and unavoidable hæmorrhage came on; he says "*probably* that which is occasioned by the placenta being fixed to the os uteri, will, for the most part, not come on till the full term of parturition, when the uterus begins to dilate from the approach of labour;" which is contrary to the history we have just given, as well as to the experience of almost all the writers\* upon this subject. Besides the very economy of the uterus makes our account correct.

Therefore, when the full time has arrived, the woman may be surprised by a sudden and an alarming issue of blood, without the smallest premonition it is about to take place—for it sometimes makes its approach so rapidly and so insiduously, that the patient may be attacked in the midst of her domestic duties, or while in the enjoyment of company. At other times it is preceded by slight and distant pains,† and when this is the case the discharge for the most part is neither so sudden nor so alarmingly extensive—for hæmorrhage is never so overwhelming nor appalling, as when the os uteri silently and rapidly yields,

\* See Leroux, Kok, Baudelocque, Denman, Burns, &c.

† When this species of flooding is accompanied by pain, it will in general be found, that the waste is neither so sudden nor so profuse as when none attends, though each contraction of the uterus augments for the time being the hæmorrhage. It must, however, be observed, that, in proportion to the discharge, will be (*cæteris paribus*) the diminution of uterine force—and hence the infrequency of natural deliveries in this kind of flooding. Indeed the pains seem almost to cease, or in other words, the contractions yield almost as soon as they commence

and in an instant exposes a thousand bleeding vessels.\* The blood flows in an almost unceasing stream, till the woman becomes much weakened and faint; coagula may then form, and a temporary truce ensue; but this in general is both treacherous and but of short duration, especially if pains attend; for the coagula which had partially arrested the hæmorrhage are now driven away by the contractions of the uterus, or by the operation of some other cause, as accidental as unavoidable, and the discharge is renewed with perhaps even increased violence; and in this way do things proceed until the poor sufferer is either exhausted by the waste of blood, or till she be relieved by the judicious and successful interposition of art.

Where the discharge is so extensive and sudden as we have just described it to be, no time should be lost before it be ascertained, whether the flooding proceeds from a separation of a portion of the placenta remotely situated from the os uteri, or from this mass being placed over it—the symptoms which designate these different situations, though perhaps pretty strongly marked, are not sufficiently accurate to render unnecessary a more certain and decided examination. We should, therefore, upon such occasions, always examine the mouth of the uterus with great care and circumspection. In conducting this, the finger, merely introduced into the vagina, will rarely be sufficiently accurate to prevent all error; the hand should be conducted into this canal, that the utmost freedom may be given to this important examination. A proper moment, however, should be chosen for this purpose, that no evil may result from this operation—for we have just remarked, that a suspension of the discharge is sometimes effected by a eoagulum within the vagina or mouth of the uterus, which being removed in making the examination, may renew

\* This circumstance, however, rarely obtains, but where the woman has arrived at, or very nearly at, her full term, and where she has been teased by some previous discharge. May not the pretty constant, though inconsiderable discharge just noticed, contribute to this sudden dilatation by acting as an uniform local depletion?

the flooding to the decided injury of the patient—while the blood is flowing, is the time to make this attempt.

When the hand has possession of the vagina, a finger should be carried within the os uteri; it should then carefully search for, and ascertain the nature of the substance presented to it—if it be the placenta, it can be easily distinguished from a coagulum (the only thing that has any resemblance to it,) by the following characters—1st. The placenta always presents a fibrous structure of pretty considerable firmness. 2dly. When this is pressed upon by the extremity of the finger, a sensation of tearing an organised substance is excited. 3dly. It being much firmer in its consistence, and offering more resistance to the play of the finger within it. 4thly. Its not escaping from the finger when its substance is in some measure broken down by the pressure and free movement of it—it can never be mistaken for the membranes.

In a case of such importance, we should neither permit a false humanity, nor a false delicacy, to get the better of an imperious duty—for upon the early knowledge of the species of flooding, the woman's life may unquestionably depend. We should therefore, without reserve, state to the friends of the patient, our opinion of the nature and tendency of the case, and the importance of ascertaining it by a proper examination. This will almost always be cheerfully acquiesced in, and if it be properly conducted, we shall neither excite any severity of pain, nor wound the most fastidious delicacy. The hand for the most part, from the relaxation consequent upon a constant discharge, will pass without difficulty, or may be made to do so by proper lubrication. It is true, indeed, that with a first child and at an incomplete period of utero-gestation, there may be some difficulty in passing the hand, if the discharge has not been pretty abundant—but in this case the examination is not so immediately important—but should it be so, from the excess of the hæmorrhage, then the parts will be found almost always sufficiently yielding to permit the passage of the hand without difficulty.

Having determined it to be a placental presentation, the condition of the mouth of the uterus should next be cautiously examined—the degree of opening, and its disposition or indisposition to dilate, should be carefully marked ; for on this much depends. It will be found in one of the following situations :—1st. But little opened and very rigid. 2dly. But little opened, yet disposed to dilate. 3dly. Opened to some extent, but very unyielding. 4thly. Opened to the same extent, but soft. 5thly. Fully dilated.

The nature of the case being thus ascertained, the mode of treatment is next to be considered. This will necessarily be much influenced by the particular condition of the woman, and the period at which the discharge may show itself, and make interference necessary. We have already noticed that when the placenta is situated over the mouth of the uterus, slight discharges of blood may take place after the sixth month, as an inseparable consequence of the economy of the uterus at this period ; when these are moderate, they may for the most part be arrested by the means usually employed for this complaint when the placenta is not placed over the mouth of the uterus—and they should be put into immediate requisition, and the patient placed under the strictest injunctions of obedience and conformity to directions. For a discharge of blood at this period is always to be looked upon as being capable of extreme augmentation, and we should never lose the suspicion, that it may arise from the situation of the placenta. We have no decided mark by which the “accidental” may be at this time distinguished from the “unavoidable,” unless we make a full examination—now, this can never be necessary so long as the flooding is moderate ; we think, however, we have observed in the “unavoidable,” that the flow of blood is more sudden and copious, in a given time ; and is more fluid and florid than in the accidental ; and in the commencement, is never accompanied by coagula ; and when pain attends the discharge, is always increased at each contraction ; but in cases demanding precision,



these marks cannot be relied upon—from the proximity of the bleeding vessels to the os externum, the blood will issue so quickly from the injured vessels, as to appear both more fluid and more florid, than in the accidental species; for in the accidental, the blood may escape remote from the os uteri, and be obliged to travel slowly through the meshes of the connecting medium of the ovum and uterus; and hence will appear less florid and fluid, and be more disposed to coagulate. But coagula will form in the “unavoidable,” when the discharge is about to cease, either by proper treatment, or by the mere efforts of nature; and it is but in this way that a stop is put to farther waste.

As we cannot in the commencement of these early discharges, determine the situation of the placenta without much pain and force, it may always be well to treat them as if they were cases of placental presentations, as in doing so we shall be erring on the safe side—we should insist upon the most perfect tranquillity of the body and mind, and an almost constant indulgence whenever practicable, in an horizontal position. Blood at this period may be taken from the arm, if the arterial force be too great; cold applications should be resorted to, and the sugar of lead be exhibited in sufficient doses either by the mouth, or by enemata as already advised. Kok and others recommend cold astringent injections to be thrown into the vagina, of the utility of which, as we have said before, we have much doubt—at least we have never been much tempted to employ them. We rarely pay any attention to the state of the bowels, unless they be very costive—then a mild, warm injection of molasses and water, or soap and water, will be every way sufficient. We are thoroughly convinced that much mischief is frequently done by the exhibition of even the very mildest purgatives; and the reason will be immediately obvious, when we consider the effects of them. We have frequently permitted our patients under treatment for uterine hæmorrhage to be five or six days

without a discharge from the bowels, and when we have thought it necessary to stir them, it has for the most part been by mild injections.

Kok also advises the introduction of some substance, such as lint or rags, into the vagina, well imbued with a fluid styptic, such as a strong solution of alum, or of wine in which alum is dissolved. We should place more reliance upon this latter means than upon the former: as it would in some measure act as a tampon, and serve as a point *d'appui* for coagula to form upon; for at last we must have these to form, if the hæmorrhage stop without having done much mischief. We have advised bleeding when the pulse is active; Kok says this is useless, if not injurious, in this kind of flooding. But in this we cannot altogether agree with him, at this period of utero-gestation, and for the following reasons:—1st. Under any kind of active hæmorrhage, when the pulse is vigorous, the taking away blood from the arm has uniformly been found useful, by producing contraction by the mere unloading of the vessels, and more especially in diminishing the velocity of the blood within them. 2dly. At the period we are speaking of, as we cannot, from the contingencies just mentioned above, decide with certainty that the discharge is from the peculiar location of the placenta, without manifest violence, we may act as far as the bleeding is concerned as if it were an “accidental” hæmorrhage, especially as the blood detracted will not seriously weaken the woman, and as there is a strong probability that it may be arrested until the last period of pregnancy, by proper applications and treatment. 3dly. That at the time this accident shows itself, it is for the most part from the mechanical separation of a portion of the placenta, which will not generally be renewed for some time, as these vessels, and the other connecting media, possess considerable elasticity, therefore time will be given for the formation of coagula, provided the proper means be pursued to favour their production, among which we must reckon bleeding. 4thly. If the fluor be not produced by external violence, or any other cause which will certainly

excite the action of the uterus, there is a strong probability that the discharge will cease for the time being, unless it be improperly treated, or unnecessarily provoked.

Should any cause whatever excite the contraction of the body and fundus of the uterus at this period of pregnancy, and the discharge be rather the effect of such contractions, than the natural and unavoidable stretching of the neck, we have great reason to fear, that we shall not be able to suspend these efforts, so as to enable the woman to go her full term of gestation. But we should ever have this intention in view, as it may sometimes be happily fulfilled; and if it be not, it is decidedly the most proper mode of treatment. In such cases, we should endeavour, as quickly as possible, to interrupt uterine contraction; for this purpose, we should bleed under the restrictions just mentioned; we should exhibit the sugar of lead with laudanum, as frequently as the exigencies of the case may require; and by enemata, we think, is much the most prompt and efficacious mode of administering them. From a scruple to a half drachm of this salt, with a drachm of laudanum, and a gill of water, may be thrown up the rectum every hour, or more seldom, as occasion may call for them. All the auxiliary plans we have already recommended should be put in requisition, and their full adoption rigidly enforced. Should these means moderate the discharge, and the blood be found disposed to form coagula; and if at the same time uterine contractions have ceased, or even to have considerably diminished, we may be encouraged to persevere in the use of the remedies we are employing, and at the same time entertain an expectation of future success. The introduction of a moderate sized tampon at this time, as a mere point d'appui, we are persuaded is highly useful, for without some such support, the coagula may be discharged, and the hæmorrhage renewed.

The artificial support for coagula, of which we have just spoken, is of more consequence than we should at first sight imagine. It permits the thinner parts of the blood to escape through the meshes of the sponge, by which means

the coagula are rendered more firm and tenacious, besides diminishing, by a counter action, the influence of the vis à tergo, which is constantly operating to throw them off. We are aware that some would rely upon the coagula without it, and we must admit that they have occasionally been sufficient to save the woman's life ; a case of this kind is related by Leroux,\* but he expressly declares they are not to be relied upon. The sudden movement of the woman's body for even necessary purposes, uterine contraction, the escape of the waters, &c. may all serve to derange the coagulum which has arrested the hæmorrhage. But the most important use of the tampon under these circumstances remains to be mentioned, which is that it causes the coagulation of the blood, merely by presenting a surface favourable to this change, long before this disposition would otherwise successfully show itself ; for in general this effect is not produced, but when the woman is much exhausted, or by the rather tardy though successful influence of the remedies previously employed.†

Should all our endeavours, however, fail to arrest the discharge, we should, without further loss of time, ascertain the condition of the os uteri, and then proceed precisely as if the woman had arrived at her full time when the hæmorrhage commenced ; for it will now be found, they are reduced exactly to the same condition, and will require the same mode of management ; of which we shall speak more at large presently.

A woman may escape these anticipating discharges until she complete her full term ; but at this time it will be seen that the uterus cannot expel its contents without *necessarily* exposing the patient to the most imminent risk. So alarmingly profuse, and so suddenly dangerous, are these discharges in some instances, that a few minutes are sufficient to exhaust the strength, or deprive the woman of existence.

\* Observations, p. 258.

† Mr. Burns assures us he never saw a case which required delivery during the first paroxysm, if a proper treatment had been adopted—Principles of Midwifery, 5th edition, p. 323.



We once witnessed a case in which there was discharged from the uterus, in the course of about fifteen minutes, a full half gallon of blood ; and we were sent for in another instance where the woman had expired before our arrival, though there had not, as the midwife assured us, more than a half hour passed from the commencement of the flooding to its fatal termination. These are, however, extreme cases, yet they show how suddenly and certainly they may be alarming or fatal. It is confessed, upon all hands, that no accident attendant upon conception is equally menacing as the disease in question, and emphatically declares to the physician, that upon him much depends, that it shall not be very often fatal. It is one of those extraordinary cases in which nature does less for the preservation of the individual than almost any other. This does not arise so much from want of exertion, if we may so term it, as from the almost entire incompatibility of giving birth to the child, and affording safety to the woman, at one and the same time. Yet we learn from such authority as cannot be doubted, that the woman, left entirely to herself, will not always perish. The mode, however, in which nature affords this security, neither offers to us any practical hint, nor holds out the smallest inducement to imitate her ; for the very means are so entirely contingent, and sometimes so long withheld, that the woman, from her great exhaustion, can scarcely be said to profit by the interposition.

Baudelocque\* says the woman may be preserved "when the orifice is fully dilated, and the mass separate entirely from it, and be so far removed from one side, that the membranes may present. The membranes may then tear spontaneously, and delivery be performed naturally, if the woman, notwithstanding her loss of blood, still preserves sufficient strength, as has sometimes happened." Leroux† by the formation of coagula and the spontaneous action of the uterus. Smellie‡ to the entire separation of the pla-

\* System of Midwifery, Vol. II. par. 986.

† Observations, &c. p. 306.

‡ Midwifery, Vol. II. Col. 18. No. 3. Cases No. 3, 4, 5, 6, 7.

centa, rupture of the membranes and the placenta being first delivered, &c. &c.

From this it would appear, that in some rare instances the woman has been saved, by the natural agents effecting the delivery before she was too much exhausted ; but that we do not profit by the knowledge of the manner in which this was performed. It is therefore, now completely established, that the only chance the woman has for life is by a well timed and well conducted delivery in every case, *cæteris paribus*, of placental presentation.

Though it be universally admitted, that, in the cases we are now considering there is but one certain mode of proceeding, yet it is not so generally conceded that it is essential to the entire success of that mode, that the delivery be properly timed, and as properly conducted. All who have written upon this subject, seem to agree in one of the positions, namely, that delivery is absolutely necessary ; but many, and indeed, we may add but too many, have been regardless of the conditions which serve to render this operation availing. It would be but a needless repetition, to cite authorities in proof of this, as we have already animadverted upon the same error when speaking upon the practice of forcing the uterus in the accidental hæmorrhage, when not readily disposed to yield. The same or perhaps more sudden mischief would follow this rashness in the cases under consideration, than in the former ; for the flooding would almost always be increased, in addition to the evils already pointed out.

The time *when* we shall attempt delivery, is of the greatest moment, and deserves particular investigation. Dr. Denman says, " It would be of great advantage in practice, if some mark were discovered, or some symptom observed, which would indicate the precise time when women with hæmorrhages of this kind ought to be delivered, ;" but declares " we do not at present know any such mark." Yet almost immediately after decides, that " whenever the case demands the operation, on account of the danger of the hæmorrhage, the state of the parts will on this account al-

ways allow it to be performed with *safety*, though not with equal facility." If this be true, we are certainly in possession of what Dr. D. thought so great a desideratum—for if the parts be in a condition to return with *safety*, it is certainly all that is required, when "the danger of the hæmorrhage demands the operation." For if the parts permit turning without risk, they must be in a dilated or a dilatable state, and this is all that is or can be required when the condition of the flooding "demands the operation." Then we have a rule which is never failing, when this condition of the parts obtains if it be true, that this can always be done with *safety*, if not with equal facility.

Now, it is of importance to inquire whether turning can always be performed with *safety* when the parts are in a condition to permit it; for upon this much depends. It would seem agreeably to this position, that the whole risk the woman runs in these cases arises from the "state of the parts" opposing the introduction of the hand; and when they do not, that then turning may be performed with *safety*—experience constantly contradicts this unqualified opinion; for the woman may be so far reduced, that she may expire before the operation is completed, or very quickly after.\* Besides, the opinion stated, in the manner we find it by Dr. D., would lead to the persuasion, that so long as the os uteri was not opened there could be no danger, whatever might be the quantity discharged; than which nothing can be more unfounded. For it is a well known fact, that the powers of the uterus may be so far impaired, as not to perform this office, even at the last moment of existence. In this we are supported by Rigby,† who declares, that were "this rule invariably adhered to, in some cases, it would be attended with danger, as we might wait

\* Of this we have ample proof in Cases 58, 81, 82, 89, 98, &c. of Rigby, in each of which the condition of the parts *easily* permitted turning, but not with *safety*—but we are clearly to be understood, that we attach no censure to the operation, for we are of opinion it was the only thing that could be done to give the woman a chance, and we have no question but it was properly performed. But these cases go to prove the incorrectness of the position we are now examining.

† Essay, Ed. 6th, p. 40.

for the opening of the uterus till it was too late to relieve the woman by turning the child." This will be very readily understood, when it is recollected, that the opening of the uterus mainly depends upon the longitudinal fibres acquiring the mastery of the circular,\* but when the uterus is so far exhausted of contractile power, as to remain passive, or nearly so, we shall always find the os uteri closed, (unless previously distended by an exertion of its powers) though most easily *dilatable*. We may perhaps even acquiesce in the explanation of Dr. Rigby† upon this subject, when he supposes that the position of the placenta may serve to keep the uterus closed, by surrounding its mouth, and the attachment of its fibres to this part, which is now perfectly passive and unresisting—this is both ingenious and probable.

We must now make a distinction of great practical importance, that has never, so far as we know, been attempted; which, if it be just (and our experience gives us every reason to believe it is) will in some measure serve to reconcile the conflicting opinions of writers upon the subject of the *time* when it would be invariably proper to attempt the relief of the patient by turning—it is simply this, that there is a most material difference, between the dilatation of the os uteri, or even its dilatability when effected by the natural powers of this organ, and that passive or quiescent condition which results from the languor of death. The one is the result of its organisation when its powers are not impaired or prostrated by disease, while the other is a syncope, if we may so term it, produced when these powers are exhausted by an excessive waste of blood. This distinction must constantly be kept in view, for on it depends the rational mode of treating this most formidable complaint; for if it be not, we prescribe both uncertainly and empirically. An attention to the one leads us to husband, with the utmost care, the strength and vigour of the patient; while the neglect of it makes us regardless, if not

\* See Essay on the means of lessening pain, &c.

† Essay, Ed. 6th, p. 40



prodigal of it; the one is almost always crowned by success, the other makes us constantly anticipate a doubtful issue.

We can readily account, with this distinction in view, for the difference of success in the operation of turning—when it has been effected under the dilatation of the uterus by the natural agents, it has perhaps almost always been attended by the much desired issue; but when performed, when the flaccidity of approaching death had ceased to make it difficult, it but too often was followed by the loss of the patient. Under this impression, then, we should say, that when the os uteri was either dilated or dilatable by the spontaneous operation of this organ, before the strength of the patient was materially impaired, that then, and then only, was the desirable time to operate; but, that if circumstances prevented advantage being taken at this proper moment to relieve the patient, and nothing but a choice of difficulties remain for us, we should certainly attempt to snatch the woman from her impending fate, under the cautions already suggested.

But we will attempt to put this subject in a clearer point of view, by considering what ought to be attempted for the relief of the patient under each of the conditions of the uterus above pointed out, and which necessarily comprehends every state it is at this time susceptible of.

*First, where the Uterus is but little opened, and is very rigid.*

In this situation of the uterus, all the evils we have already enumerated, when speaking of a delivery under our second division, when the uterus was in this condition, would attend a forced delivery at this time—it must not therefore be thought of, however high the authority may be that recommends it. Indeed, this has ever been a case of great embarrassment to the practitioner; and in but too many instances makes him at variance with himself, or he gives his directions so obscurely, and so hesitatingly, as to

confuse the judgment of the young practitioner.\* It has given rise to two modes of proceeding, each of which is equally wrong. The first is, to force the uterus, however rigid, provided a finger can be introduced; we have already said a good deal upon this plan, and shall only add in proof of it a quotation from Dr. Rigby,† highly illustrative of the impropriety of this outrageous practice. "In recommending early delivery, I think it right, however, to express a caution against the premature introduction of the hand, and the too forcible dilatation of the os uteri, before it is sufficiently relaxed by pain or discharge; for it is undoubtedly very certain, that the turning may be performed too soon as well as too late, and that the consequences of the one may be as destructive to the patient as the other. I am particularly led to observe this, as I have lately been informed, from very good authority, (namely, a gentleman to whom one of the cases occurred,) of three unhappy instances of an error of this sort, which happened some years ago to three surgeons of established reputation, who, from the success they had met with in delivering several who were reduced to the last extremity, were encouraged to attempt it where but very little blood had been lost, in hopes that their patients' constitutions would suffer less injury, and their recovery be more speedy; which, till the experiment was made, was a very reasonable supposition—the women died, and they seemed convinced that their deaths were owing to the violence of being delivered too soon, and not to the loss of blood or any other cause." The other is, to permit the flooding to proceed until the wo-

\* For an instance of this kind we may give even Mr. Burns himself—he tells us in one sentence, "if the hæmorrhage have been or continue to be considerable, we must not wait until the os uteri be much dilated, as we thus reduce the woman to great danger, and diminish her chance of recovery;" a few lines farther, in the same page, he says, "a prudent practitioner will not violently open up the os uteri, but will use the plug;" a little farther on he declares, "he, (a prudent practitioner) will not allow his patient to lose much blood, or have many attacks; he will deliver her immediately, for we know that whenever that is necessary, that it is easy, the os uteri yielding to his cautious endeavours." *Principles*, Ed. 5, p. 324.

† *Essay on Uterine Hæmorrhage*, p. 40, Ed. 5.

man shall be so much exhausted as to render the uterus pliant. Dr. Denman, as we have just noticed, supposed that when danger created the necessity for delivery, that then, from the loss of blood, the uterus would permit it with safety. Dr. Rigby says, that when the uterus contracts firmly round the fingers, we should desist from any attempt to deliver, and wait till the part be more relaxed by pain or discharge ; and adds, “as an encouragement, that we may safely suffer a woman to lose more blood, the contraction may certainly be looked upon as a proof that there still remains a considerable portion of animal strength, and that she has not been so much affected by the loss as we before imagined.”

We cannot recommend this plan, though it be the advice of the first authority extant upon this subject ; we are convinced from both reason and experience, that it is never (perhaps) necessary, and is always injurious—to save the woman an ounce of blood is, as we have already declared, a duty ; to save her forty or perhaps much more, is a still stronger one—to follow then the speculation of Dr. Denman, or the advice of Dr. Rigby, would be widely departing from that duty—we do not nor cannot adopt them. What is essentially important to be done in this case ? 1st. To save as much and as quickly as we can, the further expenditure of blood.—2d. To obtain, as soon as the natural powers will effect it, the dilatation or dilatibility of the uterus.—3d. To then deliver with as much speed as is consistent with the welfare of both mother and child.

The first and second of these indications are, as far as we have witnessed for the last thirty years, readily complied with by the use of the tampon, and the other auxiliary remedies—it should be instantly had recourse to, and the discharge will almost immediately abate, and in a short time be so diminished as to give no immediate concern for its effects ; by this means we not only staunch the hæmorrhage, but gain most important time ; for during this truce the natural agents of delivery will effect the desired relaxation of the os uteri. This plan, we believe, originated with Leroux,

and has been adopted with entire success by ourselves for many years past. It has also the sanction of Mr. John Burns, who recommends it by saying, "a prudent practitioner will not violently open the os uteri at an early period, but will use the plug, until the os uteri become soft and dilatable." It is true, Gardien thinks the plug will do harm, by exciting the uterus, and thus increase the separation of the placenta—but this is theory : it is not consonant with experience.

The following case, selected from several of a similar kind, will show, in a clear point of view, the promptness and efficacy of this plan. Mrs. —, while looking out of her window, was suddenly surprised by a profuse discharge of blood from the vagina. Before we arrived, though near at hand, more than a half gallon of blood was expended upon the floor and in a pot. When we saw the patient, she was upon the bed, pale, feeble, and excessively alarmed. We immediately examined her, and found the uterus rigid, and the placenta presenting. She had no pain, nor had she any previous to the irruption of blood. The discharge was very profuse and exhausting, but as the os uteri was none dilated and rigid, we instantly introduced the tampon, which was secured within the vagina by a compress upon the external labia with a T bandage. The flooding ceased immediately, and there was nothing passed the os externum but some of the thinner parts of the blood. After the tampon had been applied about four hours, pains commenced pretty briskly, and in about two hours more they were of considerable force, so much so, as to press against the external compress with some violence. We now removed the tampon, and the os uteri was found sufficiently dilated to allow the hand to pass with entire freedom—the delivery was quickly effected, and with safety to both mother and child.

For the successful fulfilment of the third and last indication, it is necessary the practitioner should be well acquainted with the condition of the uterus at the moment he is about to commence the operation—that is, he should



know how far he may rely upon its co-operation, or how far it may be capable of that degree of contraction as shall secure the woman against a farther loss of blood : this can only be presumed by taking into view, the quantity of blood lost, the debility or exhaustion it has occasioned, and the degree of force the uterus may exert at each return of pain. If then it be found that the quantity of blood be not excessive ; if the degree of exhaustion be not alarming ; and if, above all, the uterus manifest considerable vigour, the delivery may be accomplished in much shorter time, and with a much greater promise of success, (especially to the child,) than if the contrary of all this obtained ; in the latter case, the delivery must be conducted with the most cautious circumspection, that the uterus may not be too suddenly emptied of its contents, and thus augment the danger to both mother and child. We shall again advert to this subject when we come to describe the manner of conducting the operation of turning, or effecting the delivery.

*Secondly. When but little opened, but disposed to dilate.*

In this situation of the uterus, in general but few obstacles to turning or delivery will present themselves, since, if the necessity of the case require the operation, the great objection to it is in some measure removed ; for this disposition to yield to a moderate force, will secure the woman against an excessive loss of blood, by our taking advantage of this condition, and effecting the delivery in proper time. But it must be recollected, that though the uterus may be disposed to yield to a certain extent to even a moderate force, if it be slowly and judiciously applied, yet it may refuse to relax beyond this ; or to such an extent as would not embarrass the operation ; nor can it be made to yield beyond this degree, unless a dangerous or reprehensible force be applied.\* In a case of this kind, we should

\* Leroux, Mauriceau, Rigby, &c.

gain time by the employment of the tampon, as directed above, and not subject the woman to unnecessary risk by attempting to overcome the resistance by violence; and it must also be recollected, that, in cases like these, cases so replete with risk, that we are to devote ourselves to the best interest of our patients,—they should never be subjected to the chance of a fatal hæmorrhage by our leaving them even for a short time; for neither the plea of other engagements, nor a persuasion they can receive no injury during a short interval of absence, can justify our withdrawing ourselves from them: we could cite a number of instances in support of this, were such confirmation necessary. If it be judged necessary to employ the tampon, we should wait patiently for its effects; but we should wait at the bed-side, or near the person of the sufferer, that we may take immediate advantage of any favourable change in the condition of the parts, for which we were so anxiously looking; or guard against any unfavourable contingency that may suddenly or unexpectedly arise.

Sometimes, indeed, the os uteri appears entirely closed, when at the same time it is easily dilatable; this case should not be confounded with the one just considered; for here the woman may be readily delivered, should the urgency of the case require it. This situation of the uterus for the most part only takes place when the woman is almost exhausted, and the powers of the uterus so far impaired, that the agents for the *absolute dilatation* of its orifice are incapable of the effort. Should we wait for the expansion of the os uteri in such instances, we should wait in vain, and perhaps even be made to witness the death of the patient.\*

*Thirdly, opened to some extent, but very unyielding.*

Were we to consult authors upon the point of practice that would bear upon this condition of the uterus, we should find but too many to sanction a forced delivery—

\* See Rigby on Uterine Hæmorrhage.

while some others would severely reprehend it. We might employ the same arguments here as have been used against any violence being committed upon an unyielding uterus; it may sustain as much injury in the condition supposed here, as in the instances we have been considering; for if the opening be insufficient to permit the hand to pass without the employment of force, it will certainly be insufficient to allow the fœtus to pass without using much more; it should therefore be considered full as ineligible to operate in this case, as in the two we have just noticed. Besides, there is less excuse to be precipitate in this case, since the desired relaxation will most probably soon ensue, as the os uteri has already yielded to some extent; therefore by giving it a little more time by employing the tampon, the delivery may be accomplished without either violence or risk.

Had we no command of the hæmorrhage, we perhaps might be justified in the employment of force, as it would then be a dernier resource; but as we can certainly control the discharge, (as far at least as we have yet experienced,) by the tampon, we should be inexcusable to attempt delivery, until it had been properly tried, and it had failed.

But let this case not be confounded with the next condition to be mentioned; let it be certainly ascertained that the uterus, though opened to some extent, is, notwithstanding, very unyielding—a young practitioner may, in the confusion and embarrassment created by the exigency of the case, easily run into such an error; to prevent this, he should carefully examine the os uteri by placing or rather hooking his finger within it, and then drawing the edge towards him; if it readily yield to a gentle force thus applied, he may be pretty certain it will stretch by a well directed one used within its circle. But in conducting this examination, we must caution him against a mistake he may readily make if he be not put upon his guard; which is this—he may mistake the movement of the whole os uteri for a portion of it, but this error may without difficulty be corrected by deliberately performing the examination, and attending strict-

ly to the following marks—if a rigid os uteri be drawn, say towards the pubes, its edge against which the finger is placed will maintain its rigid feel, and if the finger is made to pass round the whole of its circle it will be found to be uniformly stiff and round, and not any or very little enlarged by the effort made upon it—but on the other hand, if the os uteri be *dilatable* beyond the size we suppose it to be by the touch, it will be found soft; and will yield without difficulty to the effort made to stretch it: and if the finger be then allowed to pass round it, it will be perceived to be of a lengthened shape, and to have been entirely obedient to the force employed to draw it forward.

*Fourthly, where opened to the same extent but soft.*

We have just declared, an error may be committed by an inexperienced or timid practitioner in this condition of the uterus; and we have pointed out the method by which it may be instantly corrected; it therefore behoves him not to neglect to entirely satisfy himself as to the situation of the os uteri, before he finally makes up his opinion on the proper mode of practice. A careless or ill conducted examination may in this instance lead to the loss of the patient; for, by mistaking the *absolute* diameter of the uterus for the *possible*, he may delay operating so long, as to render it totally unavailing; for we perfectly agree with Dr. Rigby,\* that however important it may be as a general rule, that the uterus must be opened to the size of a shilling or an half crown before any attempt is made to introduce the hand, yet if this rule be rigidly enforced, “it would in some cases be attended with danger, as we might wait for the opening till it was too late to relieve the woman by turning; and for this reason it seems right we should be sometimes as much influenced by the os uteri being in a state *capable of dilatation* without violence, as by its being really open.”† In our directions for the management of cases in

\* Rigby, p. 42.

† Ibid. p. 43.



the second condition of the uterus, we noticed this situation of this organ, and remarked that it usually occurred when the woman had flooded to excess—but we have known at least two exceptions to this.

*Fifthly, where fully dilated.*

When a case presents this condition of the uterus, there can be no hesitation about the proper mode of proceeding, if the exigencies of the case require instant interference; for here all objection is removed to the operation of turning, as far as any mechanical injury to the uterus is to be feared—but this is a rare case: and when it does occur it would seem to happen but under the following circumstances:—1st. In those women who are wont to have very rapid, and very easy labours. 2dly. Where the *edge* of the placenta extends over the os uteri, and where in consequence of this, the hæmorrhage has not been sufficient, though pretty profuse, to seriously injure the contractile powers of the uterus. 3dly. Where the pains have been so rapid and powerful, as to suddenly dilate the os uteri, and cause the head to carry the placenta some distance before it.

In the first case, the hæmorrhage will be of the most profuse and alarming kind; and if the woman be not very quickly aided, she will most probably die—this was the case with the poor woman who lost her life before we could get to her assistance—here, not a moment is to be lost; turning must be instantly had recourse to.

In the second instance, the discharge, though perhaps very free, is never so overwhelming as in the first, for the edge of the placenta may be passed over the os uteri but a small distance, and the flooding will of course be in proportion, as this may be more or less extensive—in these cases the membranes may even present, rupture spontaneously, and thus save the woman; here the natural agents may accomplish the delivery—but more of this by and by.

In the third case, the flooding will be perhaps for a pe-

riod as alarming, and, for the time of its continuance, as profuse as in the first—but the uterus acting promptly and vigorously, the head of the child is made to press so effectively upon the mouths of the bleeding vessels as to arrest the hæmorrhage\*—here we must act according to circumstances; if we see the patient during the time of her profuse flooding we should not hesitate a moment to deliver, even though the pains be brisk, for it is entirely contingent that the discharge will be stopped by the intervention of the head—but should we not see the patient until by the progress of the head the bleeding be arrested, we should not interfere, but commit the case to nature.

It has been recommended by some, to rupture the membranes in the expectation of stopping the hæmorrhage, as it frequently does when the placenta is not fixed at the mouth of the uterus—but this should never be done, especially before the uterus is well dilated or easily dilatable, and for the following reasons:—1st. Because they cannot be reached without great difficulty in some instances, and in these cases when they are reached, it is either by piercing the centre of the mass, or separating a portion of the placenta, and thus increase the bleeding surface. 2dly. When they are pierced and the waters evacuated, it will very rarely stop the hæmorrhage. 3dly. When it does not do this, we are sure to have the difficulties of turning increased. 4thly. That should the flooding for the moment cease after the discharge of the waters, it is sure to return as the pains increase, and as the uterus expands. The only exceptions to these rules are the cases just mentioned above, where the membranes present themselves in part.

Baudelocque assures us he never saw but one case, where the hæmorrhage ceased after the discharge of the waters, and that was where the placenta was first delivered by a midwife, and the head of the child was made to press so firmly on the mouths of the bleeding vessels as to stop the hæmorrhage.†

\* Baudelocque, Leroux, &c.

† System, Vol. II. par. 982.

It may be inquired, what mode of relief is to be pursued, in placental presentations, when they happen at or near the sixth month? These are truly embarrassing cases when they occur, as, for the most part, the uterus is not sufficiently enlarged to admit the hand to turn, and the hæmorrhage is sometimes very alarming; the great risk in these situations arises from the want of disposition in the os uteri to dilate; and before this is accomplished, the woman sometimes succumbs from the unrestrained <sup>the</sup> flow of blood. But women in this situation, even when unaided, do not necessarily die, nature being now and then competent to the task of delivery.\* We may remark as a general rule, and as a consolatory circumstance, that nature, if not interrupted, or when given the best chance, will almost always effect the expulsion of the ovum, previously to, or soon after the sixth month, without the manual interference of the accoucheur—for the most part, then, in such cases, our attention should be directed to the diminution of the hæmorrhage by such palliatives as we have constantly in our power; among these the tampon stands foremost.

This remedy should be early employed, as it will by proper management, save a prodigious expenditure of blood; we gain by its application important time; time that is essential for the successful delivery of the fœtus—for by it, the woman's strength is preserved; pain is permitted to increase, and eventually, though tardily, the os uteri is dilated, the placenta and fœtus thrown off, and the flooding almost immediately controlled. The other means which we have constantly pointed out, should also be tried; they may aid the general intentions, and render the operation of the tampon more certain.

We have the examples of Mauriceau and others, to attempt the relief of the woman by manual exertion in these cases; but we should, neither from the history of their cases, nor our own experience, be tempted to recommend

\* Rigby, Leroux, &c.

this plan. We are persuaded from our own observations, (which, however, we do not wish to be taken for more than they are worth,) that the temporising mode we have just suggested, is the proper one to pursue—Leroux long since adopted this method, and we have for many years but trod in his footsteps; and it is but just and proper to add, we have had abundant reason to be satisfied. Dr. Rigby, though by no means confident of the efficacy of the tampon, confesses in the cases we are now considering, it might be used with propriety—had he put this plan in execution, we are persuaded he would have been satisfied with its effects, and would unquestionably have prevented his giving the hazardous advice, “to wait for relaxation,” by permitting the patient to flood until the collapse almost of death, should effect it. Experience has often convinced us, that the relaxation of the os tinæ, so desirable in the cases we are now considering, will be as certainly achieved by time, as by this excessive expenditure of blood; and this time procured by the *interruption of the flooding* by the tampon. When we effect this by this means, we assuredly gain a great deal—strength is saved by saving much blood, and the woman’s future safety is almost insured; for as a general rule we may declare, that when no violence is committed upon the uterus by an attempt at forced delivery, the only thing we have to apprehend, are the consequences of the hæmorrhage.

When the woman is farther advanced, say at the seventh month, artificial delivery may most generally be effected,\* provided we do not destroy the advantages this period gives us, by improper treatment;—for instance, the rupturing of the membranes, and the consequent discharge of the waters; it should therefore be especially guarded against. An attention to this point in these cases, is more important than at the full period, notwithstanding the advice of some accoucheurs to the contrary.

It now only remains to describe the mode of effecting

\* Leroux, Rigby, &c.



the delivery, when it is judged proper it shall be performed. In doing this, we can give only general directions for the situation of the woman, as we cannot from her extreme weakness and other causes, always command the most proper or convenient; it may nevertheless be well when we have a control, to say what in our opinion is the best—that disposition of the woman's body which will give us the most entire command of the uterus and its contents, will certainly be the most convenient for the accoucheur, and also the safest for the woman; and this position is upon the back; the knees drawn upwards, by the feet being supported by two chairs placed near the bedstead and before the patient; the breech drawn over the edge of the bed so as to have the perinæum free of it; proper materials should be placed between the woman's back and the bedstead, that she may not suffer from its hardness; and the shoulders should be rather depressed, though the head may be a little elevated by a pillow.

The floor beneath the patient should be protected by a blanket, oil cloth, carpet, or any thing else which will answer the purpose—a pot should also be placed below to receive the discharges as they may drain off during the operation—the woman should be covered in such a manner as to prevent all exposure; she should be sustained in her position by each knee being supported by one of the attendants.

The accoucheur should take such measures as shall protect his clothing from injury, but should avoid all unnecessary parade of apparel—he should bare his arm and have it and his hand well lubricated. He should place himself before the patient in such a manner as will give one of his hands the entire command of the abdomen during the introduction, and turning with the other—this is a direction of more consequence than might at first be imagined; for by the hand which is applied to the abdomen we can, in great measure, counteract the force of the one which is making its way to the fundus of the uterus, and thus prevent a portion of this body, and that of the vagina to which

it is joined, from being put upon a painful stretch—we can judge by it the degree of contraction the uterus may exert, and also promote this end by gently stimulating it by moderate friction.

Many accoucheurs, and especially the British, recommend the patient being placed upon her side; we have ever found this less convenient, than the one just suggested; and have always, where the situation of the woman would, without injury, permit a choice, adopted this method. The advantages of this position are, first, we may employ either hand as may be most convenient to the practitioner, without changing the situation of the patient. Secondly, we always have one hand at liberty to co-operate with the introduced one, by placing it upon the abdomen. Thirdly, we can pass the hand more readily in the axis of the superior strait, by having the perinæum free over the edge of the bed. Fourthly, we can regulate the discharge of the liquor amnii, *ad libitum*, a matter sometimes of great moment. But it must be remembered, we are never to attempt to procure these advantages by moving the patient when that movement would be injurious to her—therefore when she is very weak or faint, we must operate as well as we can in the position we find her—this is sometimes very awkward and inconvenient, but these are of no consideration, when the life perhaps, of the poor sufferer, is to be put in competition with our ease. There is not, for the most part, much to be apprehended from merely changing the woman from her back, should she be lying upon it, to her side; but a greater change might be very mischievous; we are therefore frequently obliged to do this before we can operate, as it would be almost impossible to turn, when the patient's back and hips are some distance from the edge of the bed.\*

Should circumstances, or choice, induce us to deliver

\* We must always remember to have pressure made upon the abdomen by a judicious assistant, when we deliver the woman upon her side, as we cannot in this position, as when she is upon her back, perform it ourselves.

from the side, we always give a preference to the left, provided an election can be made. The hips should be drawn near to the edge of the bed, and made as salient almost as may be, by the flexion of the body, and the drawing up of the knees—in this position, the left hand is to be used, as with the right it would be very difficult to operate, owing to the axis of the superior strait being very much in advance. If on the right side, the right hand should be employed, and for the reason just stated.

The woman being properly placed (if in our power,) the hand should be gently and gradually introduced into the vagina, and then into the mouth of the uterus, separating the placenta and membranes from it as it advances towards the fundus—when arrived there, the membranes should be broken by pressing firmly against them; but the waters should not be permitted to escape but at our pleasure. We can command this almost always, as our arm fills up the os externum, and prevents its passing out—from time to time we permit some to escape by pressing the arm firmly against one side of the vagina, until it is sufficiently evacuated; the object of this gradual discharge of the waters, is at once obvious, as it prevents the uterus from falling into a state of atony, by its being too suddenly deprived of them. The feet are now to be seized, and the conversion made by drawing them down to the superior strait—we should now allow a little time for the uterus to contract; when we are assured it has done so, either by pains declaring themselves, by the child advancing farther into the pelvis without our exertion, or by the firm and hardened feel of the uterus through the parietes of the abdomen, we may most safely proceed with the delivery to its termination.\*

But should the woman be very much exhausted before we commence our operations, we should use additional caution in the delivery—it should be very slowly perform-

\* It is not our design to be very minute in our description of turning, as it is precisely the same as in every other instance of labour which requires this operation, and with which every practitioner of midwifery is supposed to be well acquainted.

ed, and we should have, at each step of the process, assurances, if possible, that the uterus has not lost, or rather that it possesses, sufficient contractility to render the completion of the operation eventually safe, if performed with due and necessary care.

We are advised by some, to pierce the placenta by the hand; but this should never be done, especially as it is impossible to assign one single good reason for the practice, and there are several very strong ones against it. 1st. In attempting this, much time is lost that is highly important to the patient, as the flooding unabatedly, if not increasingly, goes on. 2dly. In this attempt we are obliged to force against the membranes, so as to carry or urge the whole placental mass towards the fundus of the uterus, by which means the separation of it from the neck is increased, and consequently the flooding augmented. 3dly. When the hand has even penetrated the cavity of the uterus, the hole which is made by it is no greater than itself, and consequently much too small for the fœtus to pass through, without a forced enlargement, and this must be done by the child during its passage. 4thly. As the hole made by the body of the child, is not sufficiently large for the arms and head to pass through at the same time, they will consequently be arrested; and if force be applied to overcome this resistance, it will almost always separate the whole of the placenta from its connection with the uterus.\* 5thly. That when this is done, it never fails to increase the discharge, besides adding the bulk of the placenta to that of the arms and head of the child. 6thly. When the placenta is pierced, we augment the risk of the child; for in making the opening we may destroy some of the large umbilical veins, and thus permit the child to die from hæmorrhage.†† 7thly. By this method, we increase the chance of an atony

\* Baudelocque.

† Ibid.

‡ Dr. Denman confesses, though he recommends the searching for an edge of the placenta, and penetrating it, that, in performing the latter, "there is rather more danger of losing the child." Denman's Midwifery, Francis' Ed. p. 484.



of the uterus, as the discharge of the liquor amnii is not under due control. 8thly. That it is sometimes impossible to penetrate the placenta, especially when its centre answers to the centre of the os uteri; in this instance much time is lost, that may be very important to the woman.\*

It is a mistake to suppose we produce a greater separation of the placenta when we pass the hand between it and the uterus, than we do when we pierce the placenta; but if it were even true, it would be no objection to the method we advocate; since, both uterus and placenta are pretty firmly compressed by the arm in its passage to the fundus, and the bleeding by this means restrained; and as this is the only objection which is raised against the method we recommend, we shall consider it as completely answered by what is just now said.

Should the placenta not be found entirely detached from the uterus after the birth of the child, we should give a little time for it to separate spontaneously; and we must endeavour to promote this by friction upon the abdomen over the uterus, unless the flooding continues to be violent; it will then be proper to pass up the hand and separate it, for it may be the bulk of the placenta which keeps up the hæmorrhage, by preventing the uterus from closing sufficiently upon the bleeding vessels.

Before we proceed to the consideration of the third division of our subject, it may be well to inquire into Dr. Rigby's opinion concerning the nature of the uterine vessels; which we are content to call arteries. He says, "the uterine vessels differ very materially from arteries, and particularly in having no such power of contraction within themselves, their contraction and dilatation being absolutely dependent upon the state of the uterus. In the unimpregnated state they are so small as scarcely to be discovered; but they are well known to increase when the uterus re-

\* Dr. Rigby admits this, and declares he has "more than once found it." Rigby, p. 64.

ceives the ovum, and to grow in exact proportion to its gravidity; and when by the complete distension of it, they have acquired their utmost magnitude, their diameters cannot be lessened until the womb, being again emptied, closes them by the contraction of its whole capacity, and restores them to their original size."

There is no one circumstance in this history that would lead us to reject the idea, that a part of the uterine vessels are arteries, and for the following reasons:—1st. The spermatic and hypogastric arteries furnish the uterus with these vessels; and it is well known that they increase in proportion as gestation advances; consequently, vessels which all agree are arteries, enlarge and in almost the same degree as those within the substance of the uterus, and which are but continuations of them. 2dly. No physical difference has ever yet been discovered between them.

These two circumstances we consider in themselves as conclusive of the identity of the uterine, and the spermatic, and hypogastric arteries. If this were not so, why should these vessels enlarge in proportion to each other? And why should the spermatic and hypogastric arteries contract when delivery has taken place, without being dependent for this effect upon the "contraction of the uterus?" Now let us see what would really present itself, were Dr. R's opinion substantially true; he declares in the unimpregnated state of the uterus, that these vessels "are so small as scarcely to be perceived;" now, how should this happen if they possessed no contractile power within themselves? The mere contraction of the uterus could not alter their real capacity; it could only change their form by strongly compressing them; therefore, if what he supposes were even true, they should constantly present to us the shape of flattened cylinders, or puckered tubes; but the contrary of all this appears when we cut into the substance of the uterus, for we then find that though the vessels are "so small as scarcely to be perceived," yet, those we do see, present to us constantly a circular form.

That they do not contract during pregnancy, or immediately after delivery, as closely as arteries in many other parts of the body do, we admit, not because they do not possess contractility in an equal degree with them, but because they cannot exert it to the same extent in consequence of their peculiar connection with the general substance of the uterus—they are every way surrounded by, and connected with, cellular membrane,\* which will permit them to lessen themselves but to a certain degree so long as the muscular fibres of the uterus remain in an uncontracted state, and for this plain reason, that the sum of their power or disposition to shut themselves up, is inferior to the power which keeps them in some measure upon the stretch. But that they do diminish in size to a certain degree after being exposed by a separation of a portion of the placenta during pregnancy, we have no doubt, as the cellular membrane by which they are surrounded will from its elasticity† permit them to do so, and thus contribute to the suppression of hæmorrhage.

We must regard the uterus during gestation as in a state of coercion—every part and portion of it sooner or later is put upon the stretch; consequently the vessels entering into its substance, must enlarge with it, or put the cellular membrane by which they are surrounded upon the stretch; but as there is a most important intention to be fulfilled by their enlargement, they are found to aug-

\* We are not wishing to be understood that there is any thing peculiar in the uterine arteries being surrounded with cellular membrane, for we are aware that this obtains wherever there are arteries; we wish merely to insinuate an opinion, that they have positively less freedom than the arteries in other parts of the body, by being more closely tied by their connecting medium, and that, as we shall immediately say, for very important uses; and that they have relatively less, would appear evident, when we reflect on the immense increase they suffer during gestation, and though they may contract very considerably, yet it may be insufficient to stop their bleeding without the co-operating contraction of the muscular fibres, for the reason we shall presently assign. Levret makes the proportion of the unimpregnated uterus to that of the impregnated, to be as eleven and an half to one; now if the arteries augment, (as is reasonable to suppose they do,) in the same proportion, it will be seen how much their calibres must be reduced before their contraction alone can stop hæmorrhage.

† Bichat, *Anatom. Gen.*

ment in a ratio correspondent to the distention of the uterus ; and they are not only made to yield in proportion to the increased demand for blood, but are also kept in that state by its constant influx, to supply the exigencies of the uterus in a state of gravidity. This condition of the uterine vessels then have two causes contributing to the same end—namely, the unfolding and separating of the fibres constituting the proper substance of this organ, and the constantly increasing tide of blood which flows within it—the first, if it does not directly contribute to the enlargement of these vessels, will to a certain extent favour it, by taking off that restraint, which a state of contraction imposes upon them, and thus make them more easily obedient to the impulses of the spermatics and hypogastrics—the vis a tergo then of these vessels may be considered as essentially contributing to their distention. Hence, we can no longer recognise the almost imperceptible vessels of the unimpregnated, in the large canals, if we may so term them, in the advanced impregnated uterus.

Let us now suppose the supply to the uterine vessels to be cut off by any means whatever, and we make a section of the uterus near or at the full term of gestation and while yet occupied by the ovum—what will this section discover to us ? One of two things certainly—either the vessels small and contracted, or still, patulous, and large. If in the first situation, Dr. Rigby's opinion of their nature is at once proved to be unfounded ; if in the latter, will it not confirm the notion we entertain, that they are kept in this situation by force as above suggested ? If this be true, will not the same cause operate the same effect, when the uterus is emptied of its contents, but remains in a flaccid and uncontracted state ? And will not the same consequences follow from the same cause in both instances, namely, a discharge of blood from the separation of the whole or a part of the placenta ?\*

\* We might also insist, that should the uterine vessels be found large and patulous when the flaccid uterus was cut into, that it would not confirm Dr. R's opinion, should the explanation just offered not be admitted ; as it might be a reason-



If these statements be true, (and we sincerely believe them to be substantially correct,) it will follow, that the uterine arteries cannot contract sufficiently to stop hæmorrhage, however eminently they may possess contractility, so long, as the muscular fibres of the uterus are in an uncontracted state, because their peculiar connection with them will necessarily prevent it—and farther we believe, that this kind of union is highly important to the uterus after the expulsion of the fœtus, to enable it, or perhaps we may say to induce it, to contract, to throw off the placenta, and prevent after hæmorrhage. They perform this valuable end by lessening themselves and obliging in a certain degree, the muscular fibres to follow them—this proves an extensive and congenial stimulus, which for the most part is successfully exerted to this end—but should the contractile fibres of the uterus be indisposed, or unable, from the action of any cause capable of this effect, to manifest this power, hæmorrhage must necessarily ensue; for the arterial extremities which are exposed by the separation of the placenta will and must remain open, not because they do not possess the faculty of contraction, but because it cannot be successfully exerted for the reasons we have already assigned—therefore, for hæmorrhage to cease, requires the co-operation of the contractile fibres of the uterus; and to make them obedient to this end, is the great aim of all our exertions.

From a review of the inquiry we have just made, it will be evident, that as far as regards effects, there is but little difference between Dr. Rigby and ourselves; but as regards structure and function, there is considerable discrepancy—our object in this attempt, is the removal of error, and not the expectation of any great practical advantage, though we are persuaded some benefit may be derived from these considerations in the cure of hæmorrhages of this kind—for

able conjecture to say, that the cause which is capable of producing an atony of the muscular fibres of the uterus, may also be capable of rendering the uterine arteries passive, and that consequently this uncontracted condition of them, would tend to prove that this was really the case, rather than that they had been endowed with the power of self diminution.

upon the notion of the uncontractibility of the uterine arteries, Dr. R. condemns the use of that class of medicine we call astringents; now, we have declared, we have frequently found advantage from the sugar of lead in such cases, (and we have recommended its employment with no inconsiderable confidence,) which must be admitted to be an astringent, and one of no common power—in the *modus operandi* of this medicine we may be mistaken, but we cannot be in its effects. There may be many other substances belonging to this class, that may be equally or even more efficacious upon trial; but we are to be deterred from employing them because the uterine arteries “cannot contract of themselves.”

In entering upon the third and fourth divisions of our subject, it will be important to their consideration, that we say a few words upon the changes effected in the uterus itself, by the delivery of the child, and the expulsion of the placenta.

We regard the uterus as a hollow muscle; and, like the other hollow muscles, it has no separate or independent antagonising power; but like them, it has a compensating one within its own organisation or structure.\* And also, like

\* We say that the uterus has, like the heart, and perhaps all other hollow muscles, an antagonising power within itself, and this by its own organisation. We shall attempt to prove this by stating, that in consequence of that contraction, which we call the alternate contraction of the uterus, having taken place, a considerable portion of the blood which at that moment occupied the uterus, is driven quaquaversum into the general system; a facility for which is derived from the frequent anastomoses of the arteries and veins, and by the latter not having valves—this is proved by the diminished thickness of the uterine parietes, and by the whole surface becoming paler at the moment of contraction; this state of things continues, until this effort has ceased—so soon as this happens, (which may be longer or shorter, according to the power which governs the contraction, and the state perhaps of the muscular fibres of the uterus itself,) the vessels which had been just before deprived of a portion of their contents by the contraction, will at the moment of relaxation be but imperfectly filled, and perhaps even a genuine vacuum be induced; so that, so soon as the restraint imposed upon the whole of the uterine vessels by this contraction is taken off, the blood will instantly rush into them, to restore the disturbed equilibrium, and thus again distend these vessels; which distension will prove a stimulus to the uterine fibres, and thus induce a new contraction; and in this way would we account for the alternate pains of labour.

all the muscles of this kind, when not distended by some distracting force, will contract by virtue of some power of its own, and upon the healthy disposition of this power in the uterus, does the welfare of the woman depend in every instance of child-birth or abortion. We shall not stop to inquire, as its consideration is not immediately involved in our present investigation, whether this is a legitimate muscular contraction, or the exertion of that power common to many organic, as well as inorganic substances, termed elasticity: our own opinion, however, is decidedly made up, that the efforts the uterus makes to expel its contents, and to close itself after it has performed this office, is by virtue of a genuine muscular contraction. In the performance of these duties, two distinct powers are concerned. One is shown by its constant disposition to lessen the cavity of the uterus, whenever it may be put upon the stretch, or at least whenever the cause is removed, that placed it in this condition. The other declares itself by alternate contraction, and is, perhaps, only an exalted degree of the same power, when urged by stimuli to this exertion, as in child birth, abortion, or from any other circumstance which may require its interference, to expel a foreign body from the uterine cavity.

The first of the powers just noticed, has been termed "tonic contraction," and the second "spasmodic contraction," from its being usually, though not necessarily, attended with pain—this latter, it must be remembered, cannot take place without the former having preceded it, though the former can happen without the latter.\*

Such then is the economy of the uterus in its healthy condition, that it immediately exerts the tonic force with which it is endowed, to close upon its contents, and accommodate itself to the precise size of such contents—thus, so soon as the liquor amnii is discharged, the uterus instantly diminishes its size, by virtue of this tonic power, in the

\* See Essay on the means of lessening pain in certain cases of labour, &c.

exact proportion to the quantity of water displaced ; and so plastic is this power, that it makes the parietes of the uterus take the inequalities presented by the surface of the child ; and when this is expelled, it reduces itself so much, as to compress the remaining placenta, and force it from its attachment with itself, and eventually to expel it from its cavity ; when this is achieved, it goes on reducing itself, until it interrupts in a great measure the supply of blood from the spermatics and hypogastrics ; closes almost completely the mouths of the vessels exposed by a separation of the placenta, and thus prevents any inordinate flow or hæmorrhage.

From this it would appear, (and it is what all experience confirms,) that the safety of the woman depends almost entirely upon the healthy exercise of that power, we have just termed the “tonic contraction ;” and on the contrary, that the risk she may run in giving birth to her child, is in exact proportion to the diminished force of this power ; of course the preventing and stopping of floodings, will depend upon recalling it when absent, or upon augmenting it when deficient.

The tonic power of the uterus may be feeble, or altogether wanting—it may be lost in every portion of the uterus, or only in a part ; thus the fundus may possess it, and the body and neck be without it ; this may give rise to the inversion of the uterus—the fundus and neck may be deprived of it, while the body may enjoy it—this may occasion the hour glass contraction ; the body and fundus may be exhausted of it, while the neck retains it ; this may produce the concealed hæmorrhage. The body and fundus may be firmly contracted, while the neck of the uterus may be flaccid ; this may occasion flooding, if the placenta has been attacked in that vicinity.

The remote causes of uterine inertion, are said by Le Roux and others to be—1st. A general morbid condition of the body, as tendency to scurvy, &c. 2d. Long illness. 3d. A depraved condition of the circulating mass. 4th. Unusual laxity of fibres, as in leucophlegmatic habits, &c.



5th. Over distention from an excess of liquor amnii. 6th. Strong emotions or passions of the mind. 7th. A long protracted labour. 8th. A previous hæmorrhage. 9th. Lesions in the proper substance of the uterus itself.

But the condition of the tonic power, is far from being always regulated by the contingent situation of the system generally ; we cannot infer its absence from the debilitated state of the body at large ; nor can we calculate upon its presence with certainty, because every other function is carried on vigorously—this is a fact well known to every practical accoucheur, and should teach us this highly important caution, to act as if this power were, or easily might be expended, and to consider no woman safe from the casualty of its exhaustion, until we are assured to the contrary, by a careful examination made with the express view to ascertain it.

Fortunately for the patient, as well as for the practitioner, this power when weakened, nay even to excess, may almost always be recalled by proper means, and is almost certainly obedient to the judicious use of appropriate stimuli ; but upon the time and manner of this application much will depend, as we shall show presently.

We are now to consider such hæmorrhagies as may occur, before the placenta is expelled. And it must be constantly recollected, as we have said above, that this cannot happen, but when the placenta is in part or wholly separated from the uterus ; and that this separation is only effected in the cases we are now speaking of, by uterine contraction, unless some mechanical violence has been previously offered, which was capable of producing this effect. For so long as the placenta preserves its entire continuity with the uterus, no flooding can ensue, should this viscus be even in a state of complete atony or exhaustion.\*

\* Unless some mechanical violence has been done to the uterus, either from external impressions, or from some incautious manœuvre performed within its cavity, as in the act of turning or the injudicious use of instruments, the placenta will preserve its connection with the uterus, and there will be consequently an exemption from flooding ; but this connection may be destroyed in a moment by

As there is considerable variety in these cases, it will be well for the sake of perspicuity, to consider them under the following heads :

1st. Where there is a partial separation of the placenta, but the uterus enjoying some tonic power.

2d. Where there is a partial separation, but the uterus possessing very little or no tonic power.

3d. Where there is a partial separation of the placenta, while the remaining portion is too adherent, and the uterus contracts but feebly.

4th. Where every thing is as at 3d, but where the uterus enjoys its full power.

5th. Where there is an entire or partial separation, but the uterus in a state of exhaustion or syncope.

6th. Where there is either a partial or complete separation of the placenta, and where the body and fundus are in a state of inertia, while the neck enjoys its tonic power.

the causes just stated. Since writing the above note, an interesting case has occurred, which completely proves our position. Mrs. —, on the 23d of March, 1823, was taken at her full period with slight pains and the other marks of approaching labour—soon after these had manifested themselves, she was seized with violent vomiting, and considerable hæmorrhage; there was almost a constant effort in the uterus to throw off its contents, together with occasional increase of pain; we were now sent for, and found the patient as above stated; the vomiting returned from time to time, and whenever it did so, there was an increase of the hæmorrhage; and this also occurred when the alternate pains were on, which gave rise to a suspicion, it was a placental presentation. We ordered the patient to her bed, and upon examination, the membranes were found protruding, and the child rapidly advancing—we ruptured the membranes immediately, and the hæmorrhage was instantly suspended; in a few minutes more the child was expelled, but still born; the navel string was cut, but not a drop of blood issued from either portion of it; every effort was unavailingly made to resuscitate the child; the placenta was found loose in the vagina, and upon examining its surface, it was found covered over its whole extent with a thin black coagulum, an evidence it had been entirely separated, and the child thus made to perish. The uterus appeared to contract well, and every thing was promising for an hour; at the expiration of this time, the uterus relaxed, and a profuse discharge instantly took place; when we arrived, for we had taken our leave, the patient was very faint and extremely sick at stomach, and very restless, which necessarily augmented the discharge; we immediately commenced a pretty brisk friction upon the abdomen, the uterus soon contracted and did not again relax. Two grains of opium were ordered every two hours, until the patient should become tranquil. On the following morning she was found much recruited, and so far since (four days) has had no unpleasant symptom.

*I. Where there is a partial separation of the placenta, but the uterus enjoying some tonic power.*

In this case, the last efforts of the uterus to expel the child may occasion a partial separation of the placenta, and of course there will be a greater or less discharge of blood. 1st. As the exposed surface may be large or small. 2d. As the contractile power of this organ may be more or less perfect. 3d. As the circulation of the blood may be more or less hurried. In almost every instance, after the birth of the child, we find a quantity of blood issue from the vagina; but the young practitioner must not look upon this as an hæmorrhage, unless it continue some time, and has an evident effect upon the pulse.\* In this case he is immediately to attempt to arrest it, by soliciting an increased contraction of the uterus, by pretty briskly passing his hand over the region of the uterus, and from time to time attempting, as it were, to grasp the uterus by closing his fingers upon it.

By proceeding in this manner, he will almost instantly find the uterus harden under his hand; a coagulum of a greater or less size will escape from the os externum; a slight pain may come on, and the placenta may be thrown down into the vagina. When this contraction takes place, as it almost always does, where the woman has not been too much exhausted previously, either by a long protracted labour or disease, the discharge of blood is quickly put a stop to; the uterus diminishes much in size, and retires almost within the pelvic cavity, while the placenta is entirely detached from the uterus, or may be even expelled from the vagina. This is, perhaps, the most simple case

\* Some women will bear a much larger loss of blood with impunity than others; and therefore we are to decide upon the propriety of interference, from the effect which this loss has upon the system, rather than from the quantity which has been expended; if we do not attend to this rule, we shall be unnecessarily interfering, where the powers of the system are every way competent to the exigencies, and in other cases we may delay assistance so long as to render it unavailing.

of flooding that can occur, and we believe it never requires any other management, than the mere friction upon the abdomen; its termination may not always be so sudden as we have now stated, but it is sure to take place in a very short time, and just as fortunately as we have described it to do.

*II. Where there is a partial separation, but the uterus possessing very little or no tonic power.*

In this case, the same cause may produce the same effect as in I.; but the uterus may be in a very different condition; here there will not only be a discharge of blood in proportion to the surface exposed by separation, and the state of the circulation, but also a continuance of it, commensurate with the atonic condition of the uterus. This state may continue for a longer or a shorter time, as may be governed by the force of the remote cause which induced the atonic state of the uterus, or as it may be of easy, or of difficult removal.

In this, like every other case of flooding at this period, we should endeavour as quickly as possible, 1st. To remove the remote cause which induced the atonic state of the uterus, wherever it is either evident or practicable; 2d. To excite, as soon as may be, uterine contraction. It will be readily perceived, that we cannot have a control over some of the remote causes of inertia just enumerated, and therefore that our chief attention should be directed to the fulfilment of the second; and this should be immediately attempted by, 1st. Frictions upon the abdomen as above directed; in this we have the greatest confidence, and never fail to employ them, whether there be hæmorrhage or not, with a view to promote contraction, if the uterine globe be not felt firm upon the application of the hand to the abdomen immediately after the child is removed from the mother; and when there is a flooding, it is what we chiefly rely upon to restore the energy of the uterus; and in this we have hitherto never been disappointed—its



influence is as prompt as it is efficacious ; indeed we consider this as indispensable, let whatever other means be employed. We have never yet had the misfortune to meet with an uterus, that was insensible to this mechanical stimulus, or to have lost a patient from the immediate loss of blood ; and we can with great truth affirm, that this simple plan has constantly appeared to us, to be the chief agent in arresting the most formidable floodings of the kind we are now considering. The external face of the uterus as felt through the abdominal parietes, appears to us to be equally sensible to stimuli of the mechanical kind, as the internal surface of it, and certainly offers facilities and advantages, the cavity does not—1st. It is always at hand to be acted upon ; 2d. No risk is run by very freely stimulating it with the extremities of the fingers ; 3d. It excites but very little or no pain if judiciously managed ; 4th. No fear is to be apprehended of increasing the discharge, which is not always the case, when the hand is employed within the uterine cavity ; 5th. No danger of inducing inflammation or other injury, as may readily happen by the introduction of the hand.

This mode of arresting hæmorrhage by reviving the powers of the uterus, is not new—it was long since recommended by a Mons. Dassé,\* an accoucheur of Paris, whose method, though we do not exactly follow it, we will give in his own words. “ Il ne faut que porter les deux mains sur la region hypogastrique, et comprimer mollement le corps de la matrice par un mouvement tantôt circulaire, tantôt de droite à gauche, de gauche à droite, de haut en bas et de bas en haut. Tous ces différens movemens, sont absolument nécessaires, à cause des différens plans de fibres que s’entrecroisent et forment une espèce de réseau.”

We have just observed, that we do not follow exactly his method, though the effect is precisely the same—one hand is all that is necessary, or that can be conveniently employed ; and if this be industriously, and properly used,

\* Journal des Savans, d’Aout, 1792, p. 494.

we are persuaded that it will rarely fail. We must, however, in justice to ourselves declare, we were in the habit of employing this method long before we were aware it had been previously recommended by M. Dassé.

But in adopting this method, we are to take care we do not abandon it too soon; for it is not sufficient that we procure the contraction of the uterus, but that we maintain it in this condition for some time, by the continuance of the friction. And we would here caution the inexperienced practitioner against alarm, when almost at the instant he feels the uterus hardening and diminishing under his hand, he hears very plainly a considerable discharge of coagula, and fluid blood from the vagina; and at the same moment he finds the uterus retiring as it were from under its pressure. This discharge is but the effect of the contraction induced by his manœuvres upon the external surface of the uterus, and must be regarded as a favourable omen, as it assures us that the uterus is about to regain its powers. Perseverance is now all important; the frictions are to be continued until he has sufficient evidence of the permanency of the contraction, by noting that the uterus no longer relaxes itself, as it did most probably at the commencement of his operations.

Should this plan, however, not succeed in detaching the placenta, and stopping the flooding, we are, secondly, to deliver the placenta by the introduction of the hand within the cavity of the uterus, which must now be considered as the cause of the continuance of the hæmorrhage, by preventing the uterus from contracting sufficiently to shut up the mouths of the bleeding vessels. It will be found either partially or entirely detached; if, in the first condition, we insinuate carefully the fingers behind the loose portion of the placenta, and gently separate the adhering part; we then grasp the mass in the hand, and rotate it several times against the internal face of the uterus, with a view of more certainly procuring subsequent contraction; nor must the hand be withdrawn until this is perceived—should the uterus, however, be found to contract firmly upon the hand

immediately after it has effected the separation of the placenta, it may be gradually, but never suddenly withdrawn.\* If the placenta be found detached from the uterus, it must be withdrawn, but practising the precautions just inculcated. We must not, however, consider the patient free from all risk because the placenta is extracted; we should examine the condition of the uterus by again placing the hand upon the abdomen; if it be well contracted, it will be found hard, and about to sink within the pelvic cavity, which will give us strong grounds to believe, that the woman is about to do well; but if, on the contrary, the uterus is found large and not very firm, we have every reason to fear there will be a renewal of the flooding, and the frictions must be again had recourse to.

It must be confessed, however, the young practitioner may not be able without some farther directions, to detect the flaccid condition of the uterus, though he may be very able to perceive a contracted one—we shall therefore state, that when the uterus is not contracted, the whole abdomen appears equally soft and pliant—if the fingers be pressed backwards from the pubes, no hard unyielding tumour is perceived; and if he inquire into the state of the discharges from the vagina, he will find them, if not profuse, more abundant than they should be—when all these circumstances combine, he may be certain the uterus is in a state of inertia; and he will soon be convinced of this after he has commenced his friction upon the abdomen, (and he should not now lose a moment before he commences it,) by finding it harden sometimes suddenly, at others gradually under his hand, and presently sink when well conditioned, into the pelvis, or at least the fundus will be found below the umbilicus.

In all cases of severe flooding of this kind, we are in the habit of directing the nurse or any other intelligent woman, to perform this duty from time to time, for an hour or two

\* If the uterus gains its wonted powers, the hand with the placental mass would be expelled almost immediately from its cavity; but when this effect is even perceived, the hand should not be permitted to leave it too suddenly.

after our departure ; and more especially, should there be a return of discharge, that no evil may arise until we can ourselves again attend to the patient.

3dly. We think it best to call in every aid in such cases that may be at command ; and we frequently exhibit a few grains of the sugar of lead, with a pretty full dose of opium ; repeating the former with a diminished dose of the latter, every fifteen minutes or half hour, until we are pretty well assured it will be no longer necessary in such crowded doses—we, however, do not give up the use of the acetate of lead, unless the stomach be very sick, for at least twelve hours, though we diminish the quantity. In alarming cases we first exhibit from five to ten grains at a dose, unless contra indicated by the state of the stomach ; but when the necessity is less, we reduce it to two grains every one, two, or three hours, as the case may require. Should much pain attend, we give laudanum or opium until it is either relieved or much subdued.

It is not unusual, where the woman has sustained considerable loss of blood, for the stomach to become much deranged—vomiting or great nausea, is almost always an attendant upon it ; and when either takes place, it becomes very fatiguing and distressing to the patient. If she vomit, the exertion is so severe as sometimes to exhaust almost to syncope ; and during this act, there is almost always a greater or less discharge of blood, which at this moment can be but ill spared—if it be sickness of stomach, it renders the patient so wretched, that she cannot rest quiet for a moment together in one position ; she therefore tosses about from place to place until she is almost spent—we dread this latter condition more than an occasional effort to vomit, as it seems to interrupt the tonic contraction of the uterus, by the influence which nausea is wont to exert upon all muscular power ; as well as to induce immediate exhaustion, by producing unceasing jactitation. Nothing tranquillises the stomach under these circumstances, so far as we have observed, like opium in the solid form—a newly prepared pill, of two grains of the opium with a very small



portion of soap, to facilitate its solution in the stomach, should be given every hour or two, until the vomiting ceases, or the stomach becomes reconciled.

Should there be a too abundant discharge after the expulsion of the placenta, though not amounting to a flooding, it should be moderated by the use of the lead; and the most effectual mode of exhibiting it, is a watery solution of it with laudanum in form of enemata, unless the woman be too weak to have it administered in this manner—we have already directed the quantity, &c. when thus used.

*III. Where there is a partial separation of the placenta, while the remaining portion is too adherent, and the uterus contracts but feebly.*

A flooding may be excessive under the circumstances mentioned in this variety, and considerable time may be lost in vainly soliciting the extrusion of the placenta by frictions upon the abdomen, and efforts exercised upon the cord,\* before it is suspected that this mass may be too adherent—it is fortunately but of rare occurrence, but its management on that account should be the better defined.

We can never know with certainty, that the complication here spoken of exists, until the hand be passed into the uterus, and a proper examination made of the condition of the placenta; for this case, as far as regards common symptoms, resembles almost every variety of retained placenta,

\* Great care should be taken in every attempt to deliver the placenta by a force applied to the cord, that it does not exceed the degree it will bear; great inconvenience is sometimes experienced, from its separating at its union with this mass, when it becomes necessary to deliver it by the hand, as it is not easily distinguished from the uterus itself, especially if the hand be compressed by the contractions of this organ—should this accident, however, happen, the placenta may be known from the uterus: 1st. By the vascular plexus that spreads itself over its internal surface; 2d. By the woman not complaining of pain when this part is touched: and 3d. By the thickness of the uterine wall at this place, as may be determined by the hand which is externally applied, and the one within the uterus.

and nothing but a strict search can justify its being pronounced a case of adherent placenta; this excuse is, we are aware, but too frequently employed to justify the introduction of the hand into the uterus to bring away the after-birth by force, when it required but a little more time, or a little more address, to have it delivered by the natural agents. We frequently hear young practitioners boast of the difficulties they have encountered in delivering the placenta, but we rarely meet with an experienced one who declares the same thing.

In this country, (at least so far as our own experience will warrant the remark,) we may say, that the adherent placenta is of very rare occurrence; while in Great Britain, or rather perhaps in London, it is comparatively frequent, agreeably to the testimony of Dr. Ramsbotham.\* This condition of the placenta may be suspected, when the uterus continues large, though pretty firmly contracted; when there is a constant issue of blood, and that florid; when the placenta is not within reach of the finger, and when after a gentle force has been applied to the cord, it is found to retract, as if an elastic string had been stretched; when, then, the quantity of blood expended from the vagina would render manual interference necessary, and more especially when frictions, the exhibition of the sugar of lead, and other “appliances,” have failed to stop the discharge, or to expel the placenta, the hand should be introduced, and the separated portion of the placenta sought for—from this part the hand should take the direction of the adhering portion, and if it appear that it would require considerable force to destroy its connection with the uterus, every attempt to detach it should be instantly desisted from, and only the piece or pieces found loose, or not adhering, be removed; the remaining part must be trusted to the efforts of nature.

There will necessarily be both a difference in degree, as well as of extent, of adhesion in individual cases—while

\* Practical Observations on Midwifery, page 80, American Edition.

some may be only rather more strict than is usual, others will seem to have the substance of the placenta identified with that of the uterus—and while a small portion only may be too adherent in one case, a large one may be so situated in another ; but in every instance where there is a separated portion, there will be a discharge of blood, either fluid or coagulated, and that, in proportion to its accumulation, or the activity of the uterine fibres. These cases are almost always accompanied by pain, though not of the most severe kind ; they, however, make but little impression upon the placenta, nor do they much diminish the size of the uterus, yet with each return, there is more or less blood discharged, and the woman rendered faint by the frequency, rather than the quantity evacuated at each contraction, except where there is a large portion separated ; then, as in every other instance, she will be more quickly exhausted. In cases like these, it seems to be agreed, that nothing but putting the uterus in a condition to contract itself into a smaller compass, by the removal of such portions of the placenta as can be readily detached, will put a stop to the flooding, or even moderate it ; and it seems also well understood, that even this does not place the woman beyond danger—the efforts of nature are not always availing, and the woman dies from the mischief created by a retained portion of the placenta.

Should the discharge continue after a piece of the placenta is removed, the acetate of lead, and frictions, should be continued ; and astringent and detergent liquors should be thrown from time to time into the uterus itself, by means of a proper syringe.\* Let this case be treated with what

\* A considerable variety of substances have been proposed for this purpose, as alum and water, wine and water, wine alone, vinegar, &c.—but what has answered best in our hands, in a very few instances of this kind which have fallen under our notice, has been a strong infusion of camomile flowers, in which a lump of quick-lime has been slacked, and then permitted to settle perfectly clear—this may be used very moderately warm, three or four times a day, or oftener, if required—the common pewter syringe for enemata, with a flexible tube attached to it, answers perfectly well—the gum elastic tubes for the throat or bladder, may be very readily fixed to the extremity of this instrument. We saw in one instance, port wine and water, with a little alum, used with advantage.

address it may, it is one always replete with danger to the woman; she may sink from the pertinacity of the discharge, or succumb under fever, or other evils excited by a putrefying placenta. It is not part of our plan to speak of the subsequent treatment of such cases; we can with much confidence refer for more information upon this head, to the very able treatise of Dr. Ramsbotham, just mentioned; and we may here take occasion to say, that not only for this subject, but several others of high interest, we would recommend every practitioner of midwifery to the perusal of his work.

IV. *Where every thing is as at III., but where the uterus enjoys its full power.*

This variety is not only less frequent, but is much less dangerous, than the one just spoken of; for the uterus when enjoying its full powers, will contract, with sufficient force to prevent any serious mischief from hæmorrhage, though there may be considerable waste before the uterus is emptied of the placenta—this requires the same manual treatment in the beginning, and the same medical routine for the subsequent symptoms.\*

\* There is a variety in this division, which cannot be treated of, as strictly belonging to the subject of consideration, yet its importance in itself will, we hope, be a sufficient apology for our introducing it here—it is where the placenta is completely adherent; and the uterus powerfully contracts upon this mass, and prevents the introduction of the hand, or even of a couple of fingers, for the removal of it, were this even practicable—it is, fortunately of rare occurrence; we have seen but two cases of it, in neither of which was there the slightest flooding; indeed, scarcely a drop of blood was discharged in the one instance, and in the other, only a few small coagula were expelled, the whole amounting not to four ounces. This case must be trusted to nature; for after repeated examinations, the uterus was not found to relax sufficiently, even to attempt the removal of the placenta. This mass was expelled on the third day entire, in the one case without any unpleasant consequences; but in the other, it employed many days before it was thrown from the uterus, and then in small detached masses, accompanied with great sœtor, thirst, and fever—the patient eventually did well, though she remained weak a considerable time.



V. *Where there is an entire or partial separation, but the uterus in a state of exhaustion or syncope.*

This variety is most truly alarming, and requires the most prompt and judicious interference, that the woman may not almost instantly die. This case occurs, 1st. Where a long protracted labour has exhausted the patient, previously to delivery, and where this has been unexpectedly sudden. The uterus from previous fatigue and exertion becomes enfeebled, though capable for the moment of a powerful effort, which suddenly terminates the labour, but by this its remaining power is expended—the placenta, from the long continued and frequently repeated pains, was ready to separate, and waited but for the contraction which expelled the child, to destroy its connection with the uterus, and to fall loose or nearly so into its cavity, and thus give opportunity to the exposed vessels to pour out a torrent of blood. 2d. It takes place, and that more frequently than from the causes just mentioned, when the labour has been very rapid, and where the child seemed to be floated from the uterus, by the sudden gush of the waters—under such circumstances, the uterus is sometimes instantly deprived of its tonic power, and thrown into a state of absolute *syncope*, as it has been happily termed by Le Roux. Or, 3dly, It may arise (and it but too often does,) from the too hasty delivery of the body of the child, after the head has escaped through the os externum—we would wish here to caution the young practitioner against one of the most formidable errors that can be committed against sound practice, or just principles; for at this moment, the uterus has expended much of its power, in pushing the child thus far; and if some little time be not allowed it to recover from this state of expended strength, before the body is hurried through the pelvis, it will be sure to increase, and perpetuate the inertia, into which the uterus has just fallen from severe exertion—hence, we have always to apprehend a flooding, where the shoulders are expelled by the same effort that

delivers the head, more especially if the child be large, and the waters but very recently expended, or where the child is small, and the quantity of water great, and that but a short time discharged. Should this condition be accompanied with a partial separation of the placenta, an alarming hæmorrhage will necessarily ensue; and if with an entire one, death may be the almost immediate consequence.

When hæmorrhage proceeds from either of the causes just stated, it will be evident, that nothing but the most prompt interference, and the employment of the most active agents, can prevail against the formidable issue of blood, that pours from the vagina—no time must be lost by temporising; the woman will sink if not instantly succoured—frictions upon the abdomen should be quickly commenced, and be actively pursued; large doses of the acetate of lead and opium should immediately be exhibited—cold water poured from a height should be let fall upon the abdomen, if the frictions do not very soon recal the contractile power of the uterus; and if much faintness from the loss of blood attend, a small quantity of moderately strong brandy and water should be given every few minutes until this disposition is relieved; this will pretty soon follow its exhibition, if the means for re-exciting the uterus should be successful—fresh air should be freely admitted, but the feet and legs should be kept warm, by bottles of warm water or heated flannels; the ergot, if at hand, or readily procured, might be tried, provided nausea or vomiting do not attend.\*

But we must here repeat, our great dependence is upon the abdominal frictions; having, so far, never known them to fail. Some practitioners have introduced ice†

\* We do not mention the ergot as a remedy in uterine hæmorrhage from our own experience, but have no hesitation in believing from theory, and from the practice of others who are every way worthy of credence, that it may essentially and promptly be useful. The proper dose will be twenty grains, and repeated in fifteen minutes should the first not succeed.

† Levret, we believe, was the first who had recourse to this remedy in the manner above stated, and it has since been recommended by others; it has lately been advised by Mr. Barlow.

into the cavity of the uterus, under these circumstances, and it is said with success. For our part, we can say nothing upon the influence of this remedy, from our own experience; and were we tempted to employ this substance, we should not judge it necessary to conduct it within the cavity of the uterus, from a belief (not, however, we freely confess, confirmed by trial,) that it would be every way as effectual if it were held in the vagina.

We shall illustrate this condition, by a case taken at random from a number of similar ones—for all these cases are so much alike, as to require but one general mode of treatment.

Mrs. — was delivered by a midwife, after a very easy, but rapid labour—the placenta was very quickly delivered, as it was found, as the midwife said, loose in the vagina; a very profuse flooding immediately ensued, for which she attempted nothing, assuring the friends of the lady it was a common occurrence, and from which nothing was to be apprehended—but the patient becoming pale and faint, her friends were alarmed, and we were sent for in very great haste; when we arrived, it was said the patient had been delivered about twenty minutes, and the placenta had been extracted about fifteen of that time. When we came to the bed side, we were truly persuaded the patient was dead—no pulse could be felt, and for some time there was a suspension of respiration; syncope having just taken place; we instantly commenced a brisk friction upon the abdomen—ordered brandy and water by the tea spoonful to be given with frequency, warm applications to be made to the feet and legs—the curtains to be opened, and fresh air admitted from door and windows, and immediately sent for pills of the acetate of lead and opium. In the course, perhaps, of two minutes after the abdominal frictions were commenced, we had the satisfaction to feel the uterus beginning to harden under the hand, and every instant to acquire more and more firmness, and in about ten minutes it was found much diminished in size, and much more solid—in the act of puckering itself up, there was a large

quantity of coagula and fluid blood expelled from the vagina, which so alarmed the ignorant midwife, to whom was consigned the task of watching the discharge, that she declared the woman must die, if we did not desist from "rubbing the womb so violently;" but what to this poor creature was so alarming, was to us great comfort, and only induced us the more steadily to persevere in our plan of irritating the uterus.

The disposition to syncope was now much less, and the pulse could, by a nice examination, be felt returning to the wrist—this gradually increased in volume and force, as the faintness diminished, and in about half an hour the patient was considered out of immediate risk, provided there should be no further return of the flooding; to guard against this as effectually as we could, we directed two grains of the acetate of lead, and an half grain of opium, to be given every half hour; the frictions upon the abdomen to be renewed, should the uterus be found to relax ever so little, and for this end a very intelligent lady who was present, was instructed to perceive any change of this kind, that might take place—the brandy and water to be given only *pro re nata*, and the most perfect rest was enjoined, though the position of the patient's body was a very constrained one. We again saw our patient in about two hours, (having given orders to be instantly sent for, in case of any unfavourable change, before we returned,) and found her situation in every respect improved; she had had no return of hæmorrhage, but was occasionally troubled with after pains—her faintness had gone off entirely, and her system was re-acting with considerable force—her position was now altered very much to her satisfaction; the brandy and water was forbidden, and she was permitted, instead of it, to take a few spoonfuls at a time of tapioca, seasoned with lemon juice and sugar—the pills of the acetate of lead were directed once in two hours. From this time her recovery was as rapid, as such a prodigious waste of blood would permit; milk was formed in sufficient quantity, after rather a longer period than usual; and



the only subsequent inconvenience she experienced was the head ache, which so almost invariably follows excessive uterine hæmorrhage; this was relieved by keeping the bowels freely open.

VI. *Where there is either a partial or complete separation of the placenta, and where the body and fundus are in a state of inertia, while the neck enjoys its tonic powers.*

This is the most insidious situation in which the uterus can well be placed; and it is one in which young practitioners have more frequently lost patients from hæmorrhage, than any other, after the birth of the child. The neck of the uterus enjoying its powers, at a time that both fundus and body are in a state of inertia, will give rise to such an accumulation of blood within the uterine cavity as will destroy the patient, should this condition be accompanied by either a partial or total separation of the placenta, without its being suspected that such discharge is going on—in this case, the hæmorrhage will be concealed; for a coagulum being arrested at the os uteri, in consequence of its contraction, will prevent either fluid blood or coagula from issuing, and as there is no apparent flooding, the inexperienced accoucheur rests satisfied that all is going on well; nor is he roused from this state of security sometimes, until the patient is in articulo mortis, or when, perhaps, all human aid is nugatory.

This case should warn the practitioner of limited experience, against a false estimate of his patient's security, and should teach him never to omit to ascertain the state of the uterus, by a careful examination of it through the abdominal parietes, as we have already advised. If, upon placing his hand upon the abdomen, he find the uterus voluminous but far from being hard; if upon inquiry he learn, that there is little or no discharge from the vagina; if he observe his patient become pale and faint, with an hurried breathing; if, upon touching the wrist, he find the pulse weak, frequent, or extinct, and the skin cold and clammy, he may be pretty

certain there is a concealed hæmorrhage;\* he has now not a moment to spare, that he may rescue the woman from an impending fate—he must be firm, prompt, and self collected, and instantly put in practice every remedy that may promise relief to his almost expiring patient.

He should commence by abdominal frictions; and if he find the uterus becoming harder in consequence of them, he should persevere, until he thinks it has acquired a disposition to contract—should the hardening of the uterus not be attended with a discharge of coagula, &c. from the vagina, he must conclude, either that the neck of the uterus is too resisting to be overcome by the contraction of the body and fundus, without further aid; or that these are too feeble to overcome the resistance of the os uteri, though the latter may be comparatively weak—in either case, he must attempt to give to the uterus an increase of power by removing its contents.

This must be conducted with much cautious coolness, that the remedy may not increase the evil—the frictions upon the abdomen must be entrusted to some proper assistant, and they should be kept up with persevering constancy, while the practitioner carefully inserts his hand into the vagina—should he find clots there, he should remove them, if they are not immediately forced off by the effort which will most probably be excited by the introduction of the hand. This being done, he is to insinuate finger after finger into the os uteri, and gradually attempt its dilatation; should it be very resisting, it must be cautiously overcome; and we believe, if this be properly conducted, it will never offer such opposition, as to render any considerable force necessary—perseverance in a well directed manner, we are persuaded, will overcome any resistance this part may offer, under the circumstances we are considering. When the hand has gained possession of the cavity of the uterus, the wrist should be so pressed against the side of the neck of

\* We say “pretty certain there is a concealed hæmorrhage,” for we cannot say he may be altogether certain, since a rupture of the uterus may be attended with all these symptoms.

the uterus, as to make room for the escape of any coagula or fluid blood that may be now disposed to issue—by managing in this way he may empty the uterus so gradually as almost to insure its subsequent contraction; and in this he will be much aided by the external friction. He is now to search for the placenta;\* if it be but partially detached, he must cautiously separate the remaining adhesions—when this is done with care and under the precautions above suggested, he is to remove it by rotating his hand, now grasping the placenta, against the internal surface of the uterus, until it manifest a disposition to contract; and then, and not till then, should the hand be withdrawn. Should the placenta be found entirely detached, it must be delivered with the same cautious regard to uterine contraction. After the delivery of the placenta, pressure and friction should be continued upon the abdomen, nor must these be abandoned until the contracted uterus give assurance of recovered energy.

In addition to what has now been directed, the other remedies which have been suggested should be had recourse to—the sugar of lead, ergot, and cold applications under the restrictions already proposed, should be tried—this case, and the one next to be considered, offers, perhaps, the best chances for the ergot, should it possess its reputed powers;—the brandy and water should not be omitted, if the woman be very faint and much exhausted. The after treatment will suggest itself; and after symptoms must be treated *pro re nata*.

\* It may be well to observe, that in every attempt to separate the placenta, we should, before we commence the operation, fix the uterus as firmly as it can well be done, by the external application of the unemployed hand upon the fundus—in fact, it should never be attempted without this precaution, as the operation is not only very difficult without it, but is also very uncertain—the woman, if possible, should be placed upon her back on this occasion, as we have directed for other purposes. It may be also proper to suggest another caution connected with this operation, which is, that we be certain that we have removed the whole of the placenta, except in those cases where it is expedient to leave a portion to the natural efforts of the uterus, as in the too adherent placenta. It is however, sometimes almost impossible to determine this, where the placenta is lobulated, as now and then happens. See Leroux, Baudelocque, &c.

When the placenta has been expelled, and is followed by flooding, the mode of proceeding is so similar to where this happens before that has taken place, that it will require but a few words to make its management perfectly clear. In this kind of hæmorrhage, like the one we have just been considering, it is required that the uterus should contract before it can be possibly arrested; therefore it will be necessary to employ all the means already pointed out for this purpose; and here, like in the other cases, we rest our great dependence upon abdominal frictions, the acetate of lead, ergot, cold applications, &c. Should the concealed hæmorrhage take place, it must be treated very similar to those before the placenta is expelled,\* that is, the hand must be introduced into the uterus, and the coagula suffered gradually to escape, while the uterus is gently stimulated by the hand passing cautiously over its surface; and when it is found to contract upon it, it may be slowly withdrawn; the after treatment must necessarily be the same. This case, generally speaking, is of much less difficult treatment, than where we have the placenta to contend with; and will always, so far as we have yet experienced, yield to the treatment proposed, provided a proper chance be given to their employment—it cannot be supposed they will be availing when the patient is in articulo mortis.

It sometimes, however, happens, that a portion of the placenta may be left, either entirely or partially attached to the uterus, which will give rise sooner or later to hæmorrhage—this may sometimes be immediately detected by the inspection of the placenta itself—at other times this is

\* This case is sometimes very suddenly fatal—we were once called by a midwife to visit one of her patients; but upon our arrival we found the woman dead—the midwife was much surprised, as she could not account for her death, since the “labour was natural and easy, and the placenta had come quickly away,”—we told her our suspicions of the case, which was afterwards confirmed, by opening the body—the whole cavity of the uterus was filled with blood, and was distended to nearly the size of one at the full period of gestation—the mouth of the uterus was found sufficiently closed to retain the blood discharged from the surface to which the placenta had been attached.



impossible, especially in those cases where we are under the necessity of bringing away this mass piece-meal—if this accident be discovered at once, it is best, we believe, always to remove it, unless it should be a portion that is too adherent to the uterus. Should this not, however, be discovered before the uterus has firmly contracted upon it, it will be much better to suffer it to remain, and trust to nature for its expulsion, than to run the risk of provoking a flooding, exciting a great deal of pain, or of producing inflammation. But should flooding attend, we must deliver the retained portion; and this can almost always be done, as the mouth of the uterus is generally found open or yielding, when hæmorrhage attends; but should it be found otherwise, it must be trusted to nature, and the excess of discharge moderated by the tampon—if this be employed, it will be well to renew it every twelve hours, taking care to wash out the vagina with the infusion of camomile tea, wine and water, &c. before it is replaced.

The retained portion of placenta may not, however, be suspected, sometimes, for several days after delivery; but we have right to conclude this to be the case, when there is frequent return of pains, pushing from the vagina coagulum after coagulum, and these followed by fluid blood upon each relaxation of the uterus. When the discharge of fluid blood happens in quick succession and in weakening quantities, we should immediately attend to the condition of the uterus; if it be found sufficiently yielding to admit the hand, it should be carefully introduced, and the portion detached and withdrawn.\* We may sometimes succeed in detaching it by insinuating a couple of fingers into the uterus, and moving them in a circular manner between it and the placenta, so as to loosen it, and it then may be removed either by hooking it with the finger, by the natural efforts of the uterus, or by the small crotchet

\* Baudelocque tells us he has known this kind of hæmorrhage show itself on the tenth day, and has been obliged to pass his hand into the uterus to extract it—*System of Midwifery*, vol. ii. p. 27.

we recommended for the removal of the secundines in cases of early abortion. If neither the finger nor the crotchet succeed, we must trust to nature; taking care to keep the discharge in subjection by the tampon.

The young practitioner is cautioned against treating this case with indifference; it is one not unfrequently attended with danger, and sometimes death has ensued very quickly, as La Motte and others assure us. Should he be doubtful of his own judgment in this case, let him, by all means, (as well as in every other case of danger,) request the advice of an older practitioner.

*On the means for preventing Flooding.*

Having at some length considered hæmorrhages which may accompany pregnancy, and follow delivery, let us say a few words upon the mode of preventing those which may succeed to labour, as we are of opinion that much may be done to this purpose. From what we have said it will be evident, that whatever interrupts the contraction of the uterus, or produces its relaxation after it has contracted, will occasion a flooding, provided there be a separation of a part or of the whole of the placenta; it is equally evident, that whatever will insure this contraction, or contribute to it, will either prevent or interrupt hæmorrhage from this part. Much, then, will depend upon the manner in which the last stage of labour is conducted, to ensure the future contraction of the uterus. This subject has been treated of by Dr. Denman, with much apparent interest; and he has given advice, that is neither conformable to theory, nor warranted by experience, if our own observations upon this point be correct. We shall quote his own words upon this occasion, that no error may arise from substituting other than his own language. The Doctor says, "When I have been attending women, who were prone to violent hæmorrhages after the birth of the child in former labours, I have made it a rule to keep them in an erect position, till the waters were discharged by the

spontaneous breaking of the membranes, and the child was on the point of being born. By this method it appeared clearly to me, that the uterus acted more favourably, the placenta came away more naturally, and the quantity of blood lost was often much more diminished.”\*

Now, we would ask any one at all conversant with the economy of the uterus during and after labour, how an erect position, the sudden evacuation of the waters at the moment “the child was about to be born,” can possibly contribute to the only circumstance at all available in the case under consideration, namely, the permanent contraction of the uterus? In the first place, an erect position will always be attended with a quicker circulation than a recumbent one; it will permit the waters to escape with more suddenness and rapidity than a horizontal one, and, consequently, the risk of atony must be increased. It is admitted, upon all hands, and by Dr. D. himself, in other places, that if the uterus be too suddenly emptied, there will be a risk of inertia, or, at least, of great irregularity of action; if this be so, how can the interest of the woman be improved by this practice? All writers upon midwifery declare, that the suddenly emptying of the uterus by the evacuation of the waters, and the rapid delivery of the child, are the most common causes of the atonic state of this organ; yet we are advised by Dr. D. to permit all this, with a view to the prevention of it! So far all theory is against it; we will now appeal to our own experience to prove it to be a bad practice.

There was a period of our lives at which we looked upon Dr. Denman to be the highest authority in midwifery; and at that time almost implicitly followed his instructions upon every point of practice, and consequently upon the subject in question, as being one of high importance; but in doing so, we were persuaded, from sufficient experience of the plan, that it not only did not answer the end for which it was proposed, but that it was decidedly

\* Introduction to Midwifery, Francis's ed. p. 494.

mischievous ; it was of course abandoned, so soon as we convinced ourselves of this truth, and substituted one almost diametrically opposite, with which we have every reason to be perfectly well satisfied. As it was impossible, *à priori*, to determine which patient might be attacked with a flooding after delivery, it became of consequence to follow some general rule with all, (where practicable,) by which the risk of this accident should be diminished. It therefore suggested itself, that whatever would insure, with most certainty, the tonic contraction of the uterus, would best guard the patient against the contingency of a flooding ; and what appeared to us the most rational to insure this, was to take off the distention of this viscus as gradually as possible, by the early evacuation of the waters ; and to diminish the force of circulation as much as was practicable, by making the woman preserve a horizontal posture when the pains became urgent, and to interdict stimuli of every kind, as wine or any other liquor, heat, and all unnecessary exertion.

Let us now make ourselves understood, when we say "the early evacuation of the waters." It is a fact notorious to every practitioner, that the membranes, if left entirely to the force of the uterus, would preserve their integrity in many, and perhaps in most instances, until the child was about to be pushed through the *os externum*. If this plan then were to be pursued, the uterus would be suddenly, instead of gradually emptied, and consequently the risk attendant upon this, (as agreed upon by all,) would be incurred, and the most probable consequence would be a flooding. But if, instead of this, we rupture the membranes so soon as the labour is active, and the *os uteri* sufficiently dilated or easily dilatable, we should give opportunity and time for the uterus to contract, before the child would be expelled, and thus guard against the evil we were apprehending. The uterus would, by this plan, diminish in size, in the exact proportion to the water displaced ; it would apply itself to the whole surface of the child, the inequality of which would serve as an important and



healthy stimulus, (all things being equal,) and prompt it to more certain contraction.

Daily experience proves the justness of this reasoning and practice ; for how rarely do we see a flooding follow those deliveries where the liquor amnii has been discharged even some hours previously ! and what can produce the exemption from this accident, but the uterus having sufficient time and opportunity to contract ? It is true, that this alone may not always be sufficient to protect the woman against an hæmorrhage, but we are convinced, from many years of experience, it is the principal one. The directions given in this paper for the delivery of the body of the child, after the head has escaped, and the abdominal frictions, must also be considered as matters of great moment, and should never be neglected, especially with women who are “prone to flood” after delivery.

There is a cause of hæmorrhage, which we have not noticed, namely, the “inversion of the uterus,” as we intend at some leisure moment to make it the subject of another paper.

OBSERVATIONS ON

MR. CHARLES BELL'S PAPER

ON THE

MUSCULARITY OF THE UTERUS.

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IN the 5th vol. of the Eclectic Repertory, p. 37, is a paper on the muscularity of the uterus written by Mr. Charles Bell, on which we beg to make some remarks. If we merely regard the anatomical detail it contains, we have but little more censure to bestow, than might with propriety attach to any attempt that has hitherto been made to develop the muscular structure of this all important viscus ; but not so with respect to the physiological and pathological inferences deduced from it. We cannot feel that Mr. Bell has been more successful in his description than Vesalius, Malpighi, Ruysch or Hunter—for it is confessed that these gentleman bestowed much patient labour and industry in the investigation of this part, but without producing entire conviction on the mind of any one, that either of them was correct, for, each differs from the other in relating what he believed he saw, and what constituted the structure of the uterus. When, then, men of such unquestionable talents as anatomists do not agree in their observations, we think we have a legitimate cause to doubt the *opinions* of any other on this subject, until his view shall be confirmed by repeated and carefully made dissections. “We can easily believe what we wish to be true,” is an old truism, and strictly applicable to all, perhaps, who

have attempted to demonstrate the muscular structure of the uterus: each appears to have had a pre-conceived theory to support, and his labour has been expended with a hope or perhaps a determination of finding the anatomy correspond with his opinion. They appear, first, to have settled in their minds what the mechanism of this organ should be in order to produce the phenomena of labour, and then set about dissecting to confirm it; but in this they differed widely, because they had different notions of the agents required. Vesalius imagined that three strata were necessary to this end; he accordingly describes this number—one is said to be perpendicular, another transverse, a third oblique. Malpighi supposed the phenomena could be best explained, by the fibres of the uterus having a reticulated distribution; he accordingly paints them of this form. Ruysch thought, that the fundus required the fibres to be so arranged as to form an orbicular muscle; he therefore says this part is so constructed; while the late Dr. Wm. Hunter was led to view the fibres placed in concentric circles around each fallopian tube, and in the body to have a transverse direction, &c. &c. Thus are we led to believe that a pre-conceived opinion has but too powerfully *biased* the anatomist, and led him to conjecture, rather than to discovery. For it will not be pretended that either of the men whose names we have just mentioned failed in industry or lacked in opportunities; and to have cleared up the mystery that envelopes this subject, could this have been effected by these means, would have been a triumph that either would have been proud to have won. What have they achieved? Certainly not the end proposed—for all cannot be right; and much do they differ in their statements from each other. We think it fair to conclude from this discrepancy, that they were not acquainted with the true anatomy of the uterus, at least so far as regards the distribution of its muscular fibres. We cannot help therefore recommending the prosecution of this subject to those whose opportunities may afford a chance of success, for we feel it a desideratum to be intimately acquainted with

the minute structure of this wonderful viscus; and are well persuaded much remains yet to be done before a thorough acquaintance with it will be acquired, notwithstanding Mr. Bell seems satisfied with the knowledge he has of it. He has accordingly deduced consequences from what he supposes to be the arrangement of the fibres of the uterus, but to which we cannot tacitly subscribe. We again repeat that, in our opinion, much remains to be done; nor should any one be prevented from the prosecution of his inquiries on this subject by the opinion of Mr. Burns, (*Gravid Uterus*, p. 44,) "that the course of the muscular fibres is of very little consequence to the accoucheur." We are of opinion that every anatomical fact is of importance either mediately or immediately; and to the accoucheur every thing relating to the uterus is, and always must be, of use; for if it be not useful to him, to whom will it be useful? He might as well have said in so many words, "that the anatomy of the uterus is of no importance to be understood by any one."

But in insisting on the utility of investigating the structure of this part, we are happy in being supported by the authority of Mr. Bell himself: for in p. 38, he says, "The prevailing notion that the muscular fibres of the uterus are very confused and scarcely perceptible, has prevented authors from founding rules of practice on the sure ground of anatomy. And if it be possible to place this matter in a clear light, it may banish, perhaps, a certain vagueness which is much to be regretted in so important a department of practice." In this we most cordially agree; and are happy in making use of Mr. Bell's opinions where they tend to support our own. But though we have employed these sentiments to strengthen what we wished to urge, we are not of opinion that he has succeeded better than his predecessors in placing his practical deductions in a "clear light," for we again insist much remains to be done before this subject can be well understood. We will however proceed with the more immediate object of this paper, and let Mr. Bell always speak for himself. "The most curious and



obviously useful part of the muscular substance of the uterus," says Mr. B. "has been overlooked; I mean the outermost layers of fibres, which cover the upper segment of the gravid uterus. The fibres arise from the round ligaments; and, regularly diverging, spread over the fundus, until they unite and form the outermost stratum of the muscular substance of the uterus." But what makes this "the most curious and obviously the most useful part" of the uterus, we cannot comprehend—for certainly there is nothing more curious in the distribution of the fibres of this part than of any other part, and from Mr. Bell's description of them we should rather be led to consider them less "curious," because, agreeably to him, they arise from the round ligaments and regularly diverge over the fundus. From this it would appear that these fibres are every way obvious, since both their origin and course are well defined. We would ask then, if both their origin and course be so well marked, how it has happened that they have been so long hidden from the eyes of other anatomists? For they cannot be the orbicular muscle of Ruysch; the perpendicular one of Vesalius; the reticulated one of Malpighi; nor the concentric ones of Dr. Hunter. But let it be admitted that Mr. B. is really correct on this point; is he so, in the offices he assigns them? "The round ligaments of the womb have been considered useful in directing the ascent of the uterus during gestation; so as to throw it before the floating viscera of the abdomen: but, in truth, the uterus could not ascend differently; and on looking to the connection of this cord with the fibres of the uterus, we may be led to consider it as performing rather the office of a tendon, than that of a ligament. It is familiarly known, that the subsiding of the belly in pregnancy, occasioned by part of the womb sinking within the brim of the pelvis, is the least equivocal sign of the approach of labour, and of the pelvis being of due dimensions; and in some measure this is also an assurance of the right presentation of the child. This layer of muscular substance operating on the round ligaments, is well

calculated to assist in expelling the fœtus; but also in a particular manner it is provided for bringing down the womb in the first stage of labour, and is well calculated to give the uterus and head of the child the right position with regard to the axis of the pelvis." From this it would seem that the offices hitherto assigned the round ligaments are not the true ones—that they act the part of tendons: 1st. They serve to depress the uterus in the pelvis; 2d. They assist in the expulsion of the fœtus; 3d. Their action in some measure serves as a criterion of a right presentation; 4th. They bring down the womb in the first stage of labour; 5th. They serve to give the right position to the uterus and head of the child, with respect to the pelvis, &c. Let us now inquire into the truth of the multiplied offices attributed to the round ligaments and the muscular fibres originating from them. With respect to the first we deny their exclusive agency for several reasons: 1. We believe them insufficient for this purpose; 2d. We think their direction would not be favourable thereto; 3d. Because we believe we can show that the whole mass of uterine fibres contribute to this end. We believe them insufficient, because, agreeably to Mr. Bell's own statement, they are but partially distributed, and only "cover the upper segment of the uterus," and consequently can have but a limited power on it—to be effectual they must have the co-operation of the other portion of this viscus; we cannot therefore admit them to be more "obviously useful" than any other portion. Their direction would very often preclude the possibility of their being useful in the manner indicated: for, instead of their being "well calculated to give the uterus and the head of the child the right position with regard to the axis of the pelvis," we should find that, in almost every instance except a first pregnancy, they would throw the fundus over the pubes and place the orifice at the projection of the sacrum or in its hollow. To perform the duties assigned them by Mr. B. they should possess very strong powers of contraction—but if these were exerted with due force, the round ligaments would be

put upon a severe stretch ; a situation highly dangerous to their structure. For, with the exception of Mr. B. they have been universally considered highly vascular—so much so in the estimation of some, as to be looked upon as almost entirely composed of vessels, and that the size of their vessels augment during pregnancy ; and their whole substance becomes uniformly softer. Now, can we perceive any thing in this arrangement, corresponding with the duties and structure of tendons ? Should the contraction of these fibres be as powerful as their alleged duties would exact, would there not be the greatest risk of a rupture of some of their vessels ? Certainly there would—and can we imagine nature so idly capricious as to increase this risk, by augmenting their vascularity ? That they perform their share in the expulsion of the child is admitted, as we have little or no doubt but that every portion contributes to this end. And we farther believe, that the true cause of the subsiding of the abdominal tumor is, that some time previous to the manifestation of pains which certainly indicate labour, the uterus is in a state of pretty strong, but alternate contraction ; and that this is excited in consequence, as it were, of the uterus refusing to yield any longer, after having had its fibres put to an uneasy stretch and thereby stimulated. The first effects of these contractions are felt by the blood vessels of the uterus ; they are compressed slightly and in consequence transmit less blood—the volume of the uterus is thus for the moment diminished, and will now in part enter the superior opening of the pelvis.

That this is the reason of the “falling” as it is termed, is rendered almost certain, by the phenomena now presented. If a finger be introduced into the os tincæ and gently pressed against the membranes, it will be found that they are alternately tense and relaxed, as is also the mouth of the uterus now in contact with the finger. If the hands be placed on the abdomen, the same kind of sensation is perceived—that is, we find the uterine globe occasionally harder and softer. Now, were these appearances produced

by the contractions of the fibres that originate from the round ligaments, and that spread themselves upon the "upper segment of the uterus," we should find this body alternately approach and recede from the lower strait—but this is not the case; and to account for this set of fibres producing the phenomena just mentioned, we must suppose them to be in a state of permanent action, while all the other fibres are alternately contracting. The second use assigned these fibres by Mr. Bell, we generally agree to, not because they "originate from the round ligaments and spread themselves upon the upper segment of the uterus," but because they are muscular fibres and constitute a portion of the uterus, every part of which, it is our creed, contributes towards the expulsion of its contents.

Moreover we cannot agree in the declaration of Mr. B. when he says that the sinking of the uterus within the pelvis "is the least equivocal sign of the approach of labour," for assuredly there are signs which direct the accoucheur with much more certainty, and were this the proper place several could easily be pointed out.

The third advantage said to be derived from the action of this set of fibres and tendons is, that when exerted so as to depress the uterus "within the brim of the pelvis," we have "in some measure an assurance of the right presentation of the child." We scarcely know how this idea could be suggested for a moment, let alone gravely and deliberately advanced. To make the action of these fibres an evidence of a "right presentation," is to declare a connection between "the right presentation" and their action. What evidence has Mr. B. of this? We unhesitatingly say, none; for we assert, without fear of contradiction, that the uterus falls "within the brim of the pelvis" with as much certainty when another part than the head presents, as when this latter portion of the child is to the os uteri. For there are but two reasons why this circumstance does not always obtain; first, where the uterine fibres are indisposed or incapacitated, to take on that action which in our view is essential to produce "falling;" se-



condly, where the distended uterus is too large either positively or relatively, to enter the pelvis; now, it is well known that neither of these causes have any necessary connection with the species of presentation.

The fourth office assigned by Mr. B. is that these fibres bring down the womb in the first stage of labour. Here we find a little difficulty to arrive at Mr. B's true meaning. If Mr. B. means by the first stage of labour what we presume he does, "the sinking of the womb within the brim of the pelvis," for he declares this "to be the least equivocal sign of labour," we shall merely refer to what we have said when that subject, was under examination; but if he mean the more advanced stage that most accoucheurs denominate the first stage,\* we should say that the use he assigns to these fibres would be injurious, as the lower part of the uterus would constantly be driven down before the presenting part, to the manifest delay of the labour.

The fifth use attributed to these fibres by Mr. B. is that they serve to give the right position to the head and uterus as regards the pelvis. Was the right position of the child and uterus to depend solely upon any influence these fibres could possibly exert, we very much fear that the act of child-bearing would not only be much more tedious, but also much more dangerous. For, if we are correct in our estimate of their powers, and mode of operating, we should find them, as has already been observed, drawing the uterus out of the line it should take to give facility to labour. And were we to cede to them the power of regulating the direction of the uterus, we could not possibly grant them the power of doing this for the head; since it is well known to every practitioner of midwifery, that while the membranes remain entire, the head may alter its position twenty times a day.

Mr. B. says farther that, if it were not for the mechanism he describes, "we should be at a loss to conceive how

\* Namely, the opening of the mouth of the womb; secretion of mucus; pain, and gathering, as it is termed, of waters.

the uterus, by its own action, could adjust the position of the orifice for the delivery of the child." We would ask is there any power that does this with exactitude? We should say we know no such power. Mr. B. has confessed himself no accoucheur; were he one he would have known that the uterus is subject to a displacement called "obliquity;" and that displacement obtains in a greater or less degree in almost every pregnancy. The body and fundus may incline to the right or to the left, or anteriorly, while the orifice will be found in the opposite direction; so that the axis of the fundus and mouth rarely correspond with the axis of the pelvis, nor does this departure create any serious inconvenience, as it is easily remedied when necessary, and this without the aid of the round ligaments, or the fibres emanating from them. In a word, we think Mr. B. has mistaken the offices of the round ligaments; and should the duties hitherto assigned them be not the true ones, we think those attributed to them by him, are quite as questionable.

In p. 39, Mr. B. observes that "after making sections of the substance of the womb, in different directions, I have no hesitation in saying, that towards the fundus the circular fibres prevail; that towards the orifice the longitudinal fibres are most apparent; and that on the whole, the most general course of the fibres is from the fundus towards the orifice. This prevalence of longitudinal fibres is undoubtedly a provision for diminishing the length of the uterus; or for drawing the fundus towards the orifice. At the same time these longitudinal fibres must dilate the orifice, and draw the lower part of the womb over the head of the child."

To this statement of the arrangement of the muscular fibres of the uterus we can agree; but cannot consent *entirely* to Mr. Bell's explanation of their uses. We do not believe that the longitudinal fibres serve to "draw the lower part of the womb over the head of the child," because we see no such effect during the time of pain; on the contrary we invariably find that that portion of the uterus

which surrounds the child's head, to be pushed with it lower into the pelvis during each pain, and the child retained nearly in the place to which it was driven by the alternate contractions, is owing to the tonic contraction of the uterus; and this last takes place in proportion as the uterus becomes emptied, for it is an inherent principle of this organ when in healthy condition, to lessen itself, as the distracting cause is removed; and to this latent power the name of tonic contraction has been given, to distinguish it from the one which is accompanied with pain, and called spasmodic or alternate. It is true that we sometimes see a portion of the uterus suddenly retract itself when set at liberty, after having been detained and stretched by the presenting part, but this is owing to its having been unusually elongated and compressed between the head and pelvis; when then you relieve it from this state of durance, it will sometimes instantly escape, but not always, to the sometimes great inconvenience of the accoucheur. And we think we may here challenge a solitary instance in the practice of any man, of the mouth of the uterus being literally drawn over the head of the child. That it eventually passes over it we admit, because the head must pass through it before delivery can be effected. In strict parlance then, it is the head passes through the mouth of the uterus, and not the mouth being drawn over the head. Indeed, were this strictly to take place as Mr. B. says it does, it would be attended sometimes with great inconvenience; as this part would then be left at liberty to embrace the neck of the child, and thus offer great obstruction to the passage of the shoulders.

At p. 40, Mr. B. observes that a very principal effect of the muscular action of the womb is the constringing of the numerous vessels which supply the placenta, and which must be ruptured when the placenta is separated from the womb. To us it is really a matter of much surprise, situated as Mr. B. is with every source of information at hand, that he should talk of the rupturing of vessels on the separation of the placenta from the uterus. We should

have imagined that Mr. Bell from his own observations, would have been able to correct the errors of the older writers on this subject; or had these failed, he surely might have been set right by almost any late author on the subject of midwifery. We convey no information at this day, when we say that there is no rupturing of blood-vessels by the separation of the placenta from the uterus; for it has been satisfactorily proved there is no inosculation of their respective vessels; that there is a greater or less discharge of blood after this process we admit, but this is from the mouths of vessels which remain patulous until closed by the tonic contraction of the uterus. We scarcely need ask what would be the consequence did this rupturing really take place. Inflammation, suppuration, and gangrene would be the melancholy suite in most instances of otherwise healthy labours. But if this opinion raise our wonder, how much more powerfully should it be excited at the following limited and novel view of the uterus.

Mr. B. p. 40, says "I have observed further, that although in producing contraction and thickening of a portion of the uterus, by boiling it, or by other artificial means, the fibres are made very evident, and the blood-vessels greatly constricted; yet they are not so effectually closed as in the natural contraction of the muscular fibres of the uterus. Thus," continues he, "we are led to contemplate the uterus as more peculiarly destined for the safe delivery of the secundines, than for the reception and growth of the ovum." Does the conclusion legitimately flow from the premises? Certainly not—Because the natural contraction of the uterus will constrict its blood-vessels more effectually, than boiling will, it follows agreeably to Mr. B. that the uterus is "more peculiarly destined for the safe delivery of the secundines, than for the reception and growth of the ovum!" The fitness of the uterine fibres for distension; the gradual, beautiful, and regular order of their development; the protection afforded by them as a covering; the wonderful increase of capacity in the exact proportion to the necessity; the mysterious and hidden mode



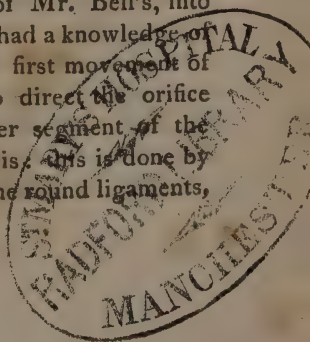
by which nourishment is prepared and conveyed ; the production of the decidua ; the engrafting of the placenta ; the almost perfect safety from external accident afforded by the waters of the amnion, all fail to make Mr. B. see that the uterus is as well calculated for the reception and growth of the ovum, as for the safe delivery of the secundines ; and why ? forsooth, because a piece of boiled uterus would not shut up its vessels as closely as if it were living.

“ I have not succeeded,” says Mr. B. p. 42, “ in discovering circular fibres in the *os tincæ*, corresponding in place and office with the sphincter of other hollow viscera, and I am therefore inclined to believe that, in the relaxation and opening of the orifice of the uterus, the change does not result from a relaxation of muscular fibres surrounding the orifice. Indeed it is not reasonable to conceive that the contents of the uterus are to be retained during the nine months of gestation by the action of a sphincter muscle. The loosening of the orifice, and that softening and relaxation which precedes labour, is quite unlike the yielding of a muscular ring.” This declaration of Mr. B. amounts but to this negative, that he did not find muscular fibres in the *os tincæ*—but does it follow they do not exist ? Do not all the phenomena exhibited by this part during pregnancy and parturition satisfactorily prove they do exist ? To what shall we attribute the resistance of this part in many cases of labour and abortions, but to the contraction of muscular fibres ? To what shall we give the power, closing to a certain extent, the mouth of the uterus during each pain, but to muscular fibres ? Or what is still more in point ; what closes the mouth of the uterus upon the finger in the beginning of labour, in the absence of pain, when we gently irritate it ? The answer we believe to be evident. Should the “ loosening of the orifice, and that softening and relaxation which precede labour,” be “ quite unlike the yielding of a muscular ring,” the contraction which almost invariably takes place with the accession of pain, certainly is not. It is completely familiar to every accoucheur, that during “ a pain” the edge of the

mouth of the womb becomes stiff and offers very considerable resistance for a long time. On what does this stiffening depend? We say upon the contraction of the muscular fibres which surround the os uteri.

Does it follow because Mr. B. thinks it "unreasonable to conceive that the contents of the uterus" can be "retained during the nine months of gestation by the action of a sphincter muscle," that that muscle does not exist? Is not the uterus, from the very important duties which it performs, entitled to its own particular powers and arrangements as well as another part of the human system? As well might Mr. B. declare the heart to be no muscle, because every other muscle has its interval of rest, because subject to fatigue, while the heart unweariedly continues its duties. Analogy may be safely and sometimes advantageously employed to strengthen an explanation, but the want of it should never be considered sufficient to destroy a fact. But would running a parallel between the powers of the os tinæ and other "muscular rings" weaken our reason for supposing this part to possess them? Certainly it would not. Thus the sphincter of the anus and that of the bladder, are intended to contract and relax at short intervals; and when in a healthy state, do completely perform their duty; while the sphincter of the uterus, for we may so term it, is designed to maintain its contraction, until that state shall be no longer necessary, and this in the healthy state is for the period of nine months. Should it relax sooner than this, more or less injury is done; but is this not precisely the case with the other sphincters? Does not mischief arise from the too frequent or the too rare relaxation of these parts.

We shall now notice another error of Mr. Bell's, into which he could not have fallen, had he had a knowledge of midwifery. In p. 42, Mr. B. says, the first movement of the uterus "is to shift its position to direct the orifice aright, and to sink down until the lower segment of the womb rests upon the brim of the pelvis. This is done by the operation of the muscular fibres on the round ligaments,



and during this shifting of its position are experienced the true dolores præsentientes." Now any practitioner could have told Mr. B. that this movement frequently takes place two or three weeks sometimes before the slightest "pain" manifests itself; and this change is effected when every thing is well disposed, without pain for the most part being, either an essential or a concomitant. And when this change does not happen, we are not to conclude that there is necessarily either a mal-presentation or a deformed pelvis, but simply to consider that some interruption has been given to the general contraction of the uterus.

As this paper has extended much beyond the length we first contemplated it would, we shall pass without notice several observations of Mr. Bell's which pretty loudly call for animadversion, that we may spend a few minutes on his "improvement" in performing the cæsarean operation. In p. 48, Mr. B. gives the history of the dissection of a case in which this operation had been unsuccessfully performed by his brother Mr. John Bell. In p. 49, Mr. B. observes that "the circumstances of this case suggest very material improvements in the manner of performing the operation." He advises "instead of cutting into the womb, as was done in the preceding case," that a very small incision should be made, such only as would enable a finger to be introduced; by boring, aided by the disposition which the uterus has to dilate, another finger might be passed, and then a third, and at length the whole hand, in a conical form, might be forced into the womb. "I further venture to suggest," continues Mr. B. "that this should be done as low down upon the lower part of the uterus, as the urinary bladder and the reflection of the peritonæum will permit; for at the lower part the uterus is least vascular, and most disposed to dilate. In the further prosecution of the operation, the child should not be suddenly extracted, but the feet being brought out by the opening, the body should be slowly delivered by the spontaneous action of the womb; and the whole operation performed as much as possible in imitation of the gradual

progress of a natural labour : the only apology for hurry in the operation would be the separation of the placenta, or the compression of the cord in the narrow wound ; but the placenta could not possibly be detached in the method now proposed, and it would be possible to guard against the compression of the cord."

We have chosen to give the words of Mr. B. at length, that no misunderstanding should arise from our relation of his "improvement." What is the "improvement" proposed? That a "small incision should be made" instead of the extensive one that has hitherto been used. Relying "on the disposition which the uterus has to dilate"\* for the amplification necessary to deliver the child, &c. through it, Mr. B. directs the incision to be no larger than "would enable a finger to be introduced," but that this is to be gradually augmented "by boring" so as to admit the hand in "a conical form." How, and at what expense is this to be obtained? Certainly not by any "disposition the uterus has to dilate" at this time for it is now at its maximum of distention. Can it be supposed that under such circumstances, a hole capable of barely admitting a finger, can be increased so as to admit the whole hand, but at the certain hazard of tearing or rupturing the substance of the uterus? If this be true (and we think it cannot be doubted) what advantage has Mr. Bell's method over the old one? We say none—on the contrary we think it highly injudicious and dangerous. It has ever been a point yielded, that much more mischief results from distracting or lacerating a part, than from cutting it by a sharp instrument; a part treated as directed in this new method of operating, would then be precisely in the condition of a

\* It is certainly new to us that the part indicated by Mr. B. for the incision (the lower part of the uterus) is more disposed to dilate than the upper part of this organ, and are at a loss to discover on what he grounds this *assertion*. As far as the order of the development of the uterus from pregnancy will aid us, we think we have a right to declare, that the lower portion of the uterus is less disposed to yield than the upper, as we uniformly find it in health to be the last that unfolds; and that this is an established provision of nature for the security of the ovum. Where this does not obtain, abortion is the uniform consequence.



contused one, and of course would have to suffer all the "penalties" arising from this outrage.

In cases where (reasoning *a priori*) we should apprehend much less injury would be done to the substance of the uterus, we are cautioned with no common earnestness by the most experienced accoucheurs not to run the risk; we mean in certain cases of "floodings" where the mouth of the uterus is not sufficiently dilated to allow the hand to pass freely. We are directed with much propriety in such cases, not to employ too much force in dilating the os tincæ, lest inflammation with its consequences should ensue. And our own observation has furnished us with more instances that one, where the most melancholy effects have followed this ill directed manœuvre. If then, mischief may arise from the attempt to dilate with too much force a part from its structure capable of being dilated, how much more is to be apprehended from an attempt upon a part not fitted by its organisation for this purpose? In the one instance we are merely stretching the fibres of a part in the direction they are known under proper circumstances spontaneously to yield; while in the other, we are obliging them, at the expense of their continuity, to give way to the distracting power of a wedge. That every evil consequent upon a severe laceration would follow this practice, we have every reason to fear; what then is gained by this "improvement?"

Hitherto we have been considering this practice under its most favourable aspect, that is, where the placenta offers no difficulty to it from its location. What then must be the objections where this mass is attached to the anterior part of the uterus? The difficulties we have just stated will not only obtain, but also those that must necessarily arise from forcibly separating this body from its attachment with the womb. For when thus situated, it must either be detached, or penetrated; if detached, hæmorrhage to a greater or less extent must ensue; if penetrated, (which by the by would be a work of much less difficulty)

much embarrassment would be experienced in extracting the child.

But let us admit that the hand has passed into the uterus and the feet seized, agreeably to the direction of Mr. B. how is the child to be extracted through the opening made for it? The child may be *forced* to pass as far as the axillæ; but how are the arms to be delivered? Will it be maintained that an aperture no more than sufficient for the hand to pass through in "a conical form," will be capacious enough, without further violence and mischief, to permit the exit of a child's body at the full period of gestation? How will the hand, or even two or three fingers be passed over the shoulders with a view to bring out the arms, since we must suppose that the artificial opening is now *completely* occupied by the body of the child? How are we to gain additional room to bring them from within the orifice of the wound? But let us suppose all these difficulties surmounted, how is the head to be relieved, or how obviate the compression on the umbilical cord? The "disposition which the uterus has to dilate," and on which Mr. B. seems almost exclusively to rely for the success of his operation, is not more remarkable, *cæteris paribus*, than its tendency to contract when freed from any distracting force. If this be true, and we think it will not be questioned, no sooner would the shoulders be liberated, than the poor infant's neck would be instantly encircled by the artificial ring intended for its emancipation, and be, to all intents and purposes, entrapped in the pillory, which had been so dexterously made by the surgeon, of its mother's womb. In vain would we obey Mr. Bell's direction to "imitate as much as possible the gradual progress of a natural labour when attempting to extract the child," for the efforts of the uterus would lend us no aid, as they would be directed in a contrary course agreeably to the opinion of Mr. B. himself; for he has confessed the action of the uterus to be from fundus to mouth. Are we to expect a miracle wrought, because this *improvement* has been resorted to? Are we to expect, that the order of this organ's

functions will be changed expressly to favour this new operation? In a word, how is the head to be relieved? We believe it cannot be delivered; unless indeed so much injury has been done to the uterus as to completely paralyse its natural powers. And in this case, we scarcely need say, it would be worse than Utopian, to expect any benefit to the mother from this operation. Where then is the advantage of this "improved" method of operating? We see none, nay, worse than none; for in the old method, both a mother and child have been sometimes saved; but in this, we have no hesitation to declare it our belief, that mother and child would inevitably perish. The risk on the part of the child is most unquestionably increased, for it must suffer all the dangers that would arise from being delivered "footling" from a small pelvis or rigid soft parts, without either the equivocal benefit of the use of the forceps. It would seem that every step we take in this new operation offers but fresh difficulties; for let us suppose the child delivered; the placenta is yet to follow. If the uterus be in a state of atony when we separate this body—for separate it we must; for Mr. B. says "that the placenta could not possibly be detached" (spontaneously) "in his method"—the woman would sink from hæmorrhage;—if it retain its powers of contraction, the artificial opening will be reduced to nearly the original size made by the knife. How in this case is the placenta to be delivered? Shall we be directed, again to dilate this aperture with the hand and then seize with it the placenta? Will no difficulty be experienced in retracting the hand when loaded with the placenta?

How are we to obviate the most common, and most obvious risk, the compression of the funis, during this tedious and difficult operation? Mr. B. has told us "it would be possible to guard against the compression of the cord," but unfortunately has not detailed the method whereby this desideratum is to be obtained. He appears to have been aware of this difficulty, but has left no suggestion to obviate it; even the old method is not exempt from this disaster,

as is proved by Mr. Bell's quotation of Mr. Hunter's case, but where he, Mr. B. roundly asserts, "in truth there is no disposition in the wound to contract," although Mr. H. was obliged to introduce a couple of fingers into the womb to protect the funis; for "the womb contracted round the neck of the child, so as to retard the delivery of the head and press the funis." (Note p. 49.) Here we find are two statements in the same paragraph in direct opposition to each other. Mr. Hunter, in relating his case, is supposed to state a fact; that "the womb contracted closely round the child's neck;" and he is quoted for the express purpose (as we suppose) to prove, that this does take place in this operation, and to bear Mr. B. himself out when he asserts that "the only apology for hurry in the operation would be the separation of the placenta, or the *compression of the cord in the narrow wound*;" yet he immediately after declares, that "there is no disposition in the wound to contract." How are we to reconcile this discrepancy in Mr. Bell, but by saying he had a theory to support?

But let us suppose the operation finished; what will be the condition of the poor, suffering, unresisting mother. Subjected by this method for perhaps hours\* to more than inquisitorial tortures, she becomes exhausted; scarcely daring to hope a life so jeopardised by severity can be saved; or perhaps is prevented even from wishing it, by the death of her infant, whose smiles were to be the humble and only reward for all her sufferings.

\* For agreeably to the instructions of Mr. B. the body should be slowly delivered by the spontaneous action of the womb; and the whole operation performed as much as possible in imitation of the "*gradual progress of a natural labour*."







